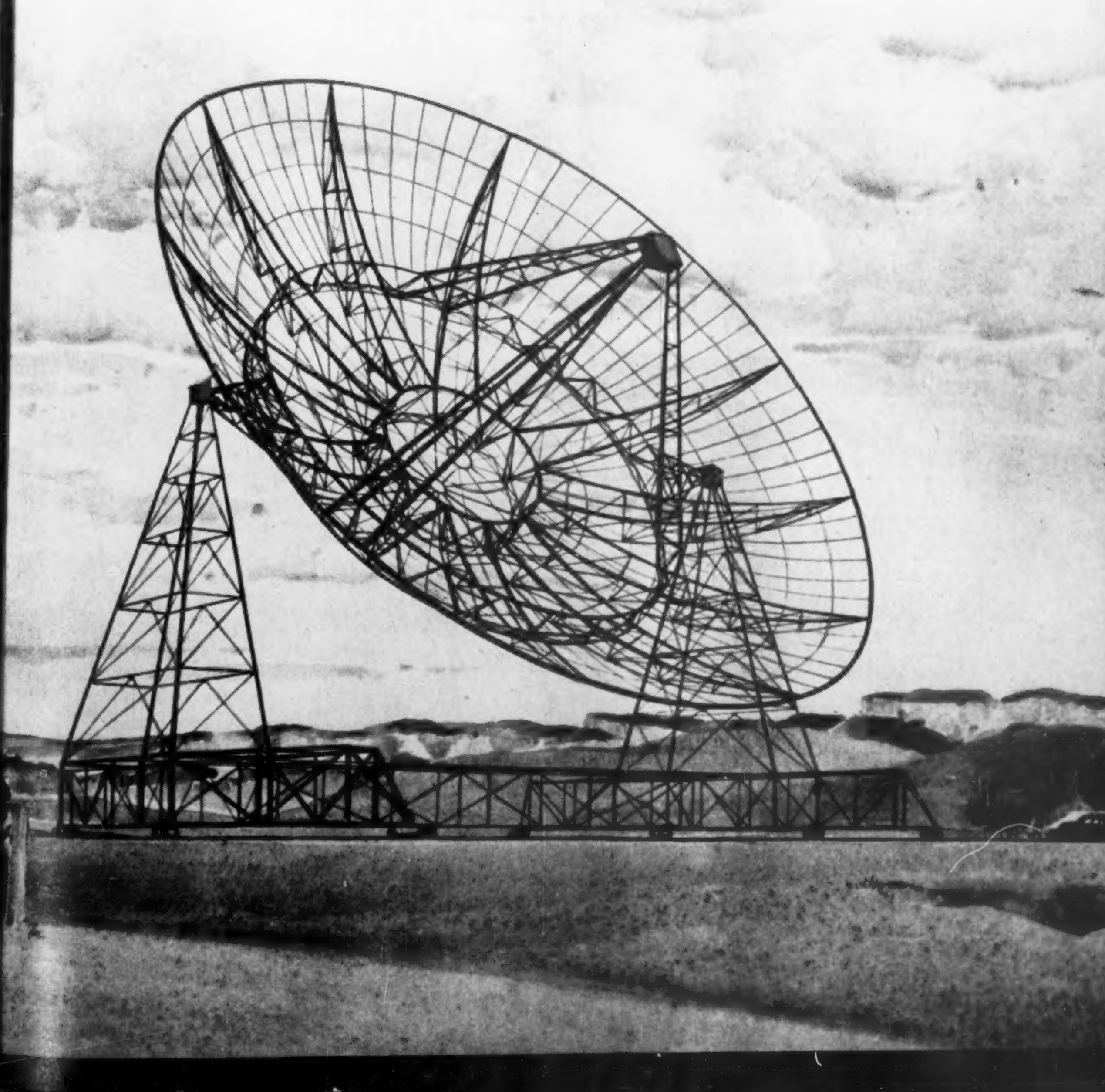
The SCIENTIFIC MONTHLY



THE SCIENTIFIC MONTHLY

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Cover: Artist's drawing of the proposed Australian "radio eye"
(Courtesy Carnegie Institution of Washington, see page 176)

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Science and Technology

(From the month's news releases; publication here does not constitute endorsement.)

Insulation Tester

Featuring a 1 to 50,000 megohm range at 500-v d-c test potential and an all-welded steel case construction, this multipurpose insulation tester has been designed to meet the needs for a versatile yet rugged insulation resistance measuring instrument for shop and field use. Closely regulated test voltage (±0.25 percent) and choice of four different megohm ranges make it especially applicable for production-line testing of capacitors, resistors, motors, transformers, wire and cable, insulating material, and so forth. (Associated Research, Inc., 3758 W. Belmont Ave., Chicago 18, Ill.)

Capacitors

A new line of glass tubular plastic dielectric capacitors feature lightweight and compactness and are particularly useful in high-voltage d-c and low-frequency, low-voltage a-c applications. (Corson Electric Mfg. Corp., Dept SM, 540 39th St., Union City, N. J.)

Radioactivity Monitor

This precision integrating instrument (Fig. 1) is a monitor for use with a Geiger counter for (i) routine surveying for alpha, beta, or gamma contamination in radioisotope laboratories; (ii) detecting large fluctuations in cosmic-ray and other laboratory "background"; and (iii) routine measurements of radioactivity for medical diagnosis and treatment, tracer work, or process control in industry. It provides four ranges to cover any radiation intensity up to 20,000 counts/min. Time constants of the rate circuit are chosen to provide the best compromise between probable error in reading and speed of response. (Nuclear Instrument and Chemical Corp., Dept. SM, 229 W. Erie St., Chiacgo 10, Ill.)

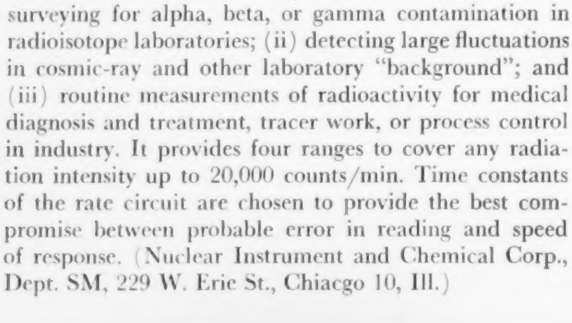

Fig. 1

Optical Pyrometer

A new type of optical pyrometer, designed particularly for precision temperature measurements in the laboratory yet sufficiently portable to be used for general plant applications, has been developed to meet the demand not only for higher degrees of accuracy but also greater versatility in the measurement of temperatures above 700°C (1300°F). It is capable of measuring targets less than 0.001 in. in diameter and, by means of supplementary lenses, can be adjusted for focal distances varying from 5 in. to infinity. (Pyrometer Instrument Co., Inc., Dept. SM, Bergenfield, N. J.)

Electronic Metals Detector

A new electronic metals detector distinguishes between metallic iron and other metals, such as gold and silver. It will pick up metals through any nonmetallic medium such as cement, plaster, wood, earth, plastic, glass, and asphalt. It will quickly determine whether painted metal objects are iron or other metals. Weighing only 3 lb, the detector can be carried and controlled with one hand. (Gardiner Electronics Co., Dept. SM, 2018 N. Dayton, Phoenix, Ariz.)

Hardness Tester

A new hardness tester makes all regular Rockwell tests (60-, 100-, 150-kg loads) and superficial tests (15-, 30-, and 45-kg loads) with any suitable standard "C," "N," or Ball-type indenter. All results show on a direct, easy-to-read dial gage with a single numerical scale. (The Torsion Balance Co., Dept. SM, Clifton, N. J.)

Commercial Quartz Shapes

Commercial quartz shapes are now available for micro and ultramicro analytic work. Items include breakers, boats, flasks, graded seals, ground joints, and capillary pipets. They are transparent and provide highest transmission of ultraviolet and visible radiations. Melting point and chemical resistance are high. (Microchemical Specialties Co., Dept. SM, 1834 University Ave., Berkeley, Calif.)

Incubator Stage for Microscope

A new convenient incubator stage for the microscope consists of a heating plate measuring 57 by 75 mm combined with an electric heating element. A variable transformer is supplied for constant controlled temperatures within the range of +35°C and 40°C. A thermometer is furnished with the unit for registration of temperature. The heating plate, which accommodates specimen slides of standard size, is easily mounted upon the stage of any standard microscope. (William J. Hacker & Co., Inc. Dept. SM, 82 Beaver St., New York 5.)

Radioactivity Scaler

A new scaler, for use with Geiger or scintillation counters in both research and clinical work involving radioactive isotopes, is capable of counting as many as 120,000 counts/min with only 1 percent "coincidence loss," so that extremely precise radioactivity measurements are possible. When used with either a Geiger or ultrasensitive scintillation counter, the instrument will be useful for diagnostic or therapeutic purposes in medical applications, qualitive or quantitative work in chemistry, routine monitoring work, and many other radiation counting problems. (Nuclear Instrument and Chemical Corp., Dept. SM, 229 W. Erie St., Chicago 10, Ill.)

Unitized Incubator

A new unitized incubator is designed to fit neatly into any lineup of unitized laboratory furniture. It is a 4-ft^3 model that holds 50 standard bottles for measurement of B.O.D. (biochemical oxygen demand) and maintains temperatures from 5°C to 50°C to within 1°C. A sensitive bimetallic thermoregulator turns heater off and on as needed; fan and baffles direct air current over and around the shelves. (Fisher Scientific Co., Dept. SM, 717 Forbes St., Pittsburgh 19, Pa.)

Tetrode Transistors

Tetrode-grown junction transistors, especially suitable for use in audio-amplifier automatic gain control circuits have two base layer connections allowing introduction of feedback. Amplification factor (alpha) of the tetrode through the amplifier section is guaranteed to be 0.95 or better. Output signal attenuation of over 20 db can be secured with less than 100 μa applied to the second base lead. (Texas Instruments Inc., 6000 Lemmon Ave., Dallas 9, Texas)

Silicone Rubber

A resilient dielectric coating or encapsulating material for extreme temperature electric and electronic components cures in 2 to 4 hr at 200° C to form a rubberlike jacket that is uniform, heat-stable, moistureproof, and highly resistant to oxidation, ozone, and weathering. Tests indicate that transformers properly coated with this new material will easily pass both moisture-resistant and low-temperature flexibility requirements of ML-T-27 grade 1. Fully cured coatings also have about twice the thermal conductivity of conventional resinous or rubber dielectrics, assuring cooler operation at high temperatures. (Dow Corning Corp. Dept. SM, Midland, Mich.)

Farmer Substandard X-ray Dosemeter

A portable, ionization-type Farmer Dosemeter has particular application in radiological clinics where dosage readings are needed for x-ray diagnosis and therapy, for health-physics x-ray monitoring, and for industrial radiography. The instrument may be easily calibrated against the built-in primary standard. The probe-type ionization chamber may be connected to the instrument by a 10-ft cable, which allows one to make repeated dosage readings at a safe distance from the source of x-rays. (Instruments & Equipment, Inc., 489 Fifth Ave., Dept. SM, New York 17)

Paper Strip Scanner

A scanning device is available for obtaining precise photometric or radioactivity readings of paper strips in electrophoresis and chromatography. Paper strips are wound on spindles at each end of the scanner in a manner similar to the loading of roll film in a camera. When the take-up spindle is turned, the paper passes the light slit of the spectrophotometer in a controlled plane perpendicular to the light beam. Density readings are taken at equal intervals on the paper as measured by an indicator knob marked for 2-mm movements. A similar arrangement of slit-window is provided in the radioactivity scanning adapter. (Research Specialties Co., Dept. SM, 1148 Walnut St., Berkeley 7, Calif.)

Video Phase Meter

The type 324-A video phase meter provides direct readings of phase angles from 0° to 360° without ambiguity. The frequency range of 20 kcy/sec to 4.5 mcy/sec may be extended down to 20 cy/sec on special orders. Angular accuracy is ±4° for either sine or complex waves having no more than one positive going zero-axis crossing per cycle. (Tippett & Co. Inc., Dept. SM, 419 Commonwealth Ave., Boston 15, Mass.)

Multichannel Oscillographs

New multichannel direct-writing oscillographs feature four- and six-channel systems. An electrically controlled chart drive system permits instantaneous speed selection. A total range of 16 accurate chart speeds is possible from 1 cm/hr to 250 mm/sec. All speeds are selected with a front-panel control or with an accessory remote-control unit. (Brush Electronics Co., Dept. SM, 3405 Perkins Ave., Cleveland 14, Ohio)

Transistor Test Set

A new transistor test set features dependable, direct measurement of transistor characteristics at any frequency from 100 cy/sec to 1 mcy/sec. Designed as a versatile laboratory instrument, its speed and simplicity of operation adapt it for routine production testing. It is used in conjunction with an external oscillator and vacuum-tube voltmeter of types commonly encountered in electronic laboratories. Provision is made for measuring phase angle as well as magnitude of the hybrid coefficients, thus permitting accurate design of AF, IF, and RF circuits. (Baird Associates, Inc., 33 University Rd., Cambridge 38, Mass.)

Geiger Counter

The new counter utilizes miniaturized components, standard-size Geiger tubes, and an electronic high-voltage supply operating on standard low-voltage batteries. (The Radiac Co., Inc., Dept. SM, 489 Fifth Ave., New York 17.)

THE

SCIENTIFIC MONTHLY

SEPTEMBER 1954

The Variety of Reasons for the Acceptance of Scientific Theories

PHILIPP G. FRANK

After receiving his Ph.D. degree from the University of Vienna, Dr. Frank was professor of theoretical physics at the University of Prague from 1912 to 1938. Since 1940, he has been a faculty lecturer in physics and the philosophy of science at Harvard University. He has also been a visiting professor at the College of the City of New York, Brown University, and Purdue University.

AMONG scientists it is taken for granted that a theory "should be" accepted if and only if it is "true"; to be true means in this context to be in agreement with the observable facts that can be logically derived from the theory. Every influence of moral, religious, or political considerations upon the acceptance of a theory is regarded as "illegitimate" by the so-called "community of scientists." This view certainly has had a highly salutary effect upon the evolution of science as a human activity. It tells the truth—but not the whole truth. It has never happened that all the conclusions drawn from a theory have agreed with the observable facts. The scientific community has accepted theories only when a vast number of facts has been derived from few and simple principles. A familiar example is the derivation of the immensely complex motions of celestial bodies from the simple Newtonian formula of gravitation or the large variety of electromagnetic phenomena from Maxwell's field equations.

If we restrict our attention to the two criterions that are called "agreement with observations" and "simplicity," we remain completely within the domain of activities that are cultivated and approved by the community of scientists. But, if we have to choose a certain theory for acceptance, we do not know what respective weight should be attributed to these two criterions. There is obviously no theory that agrees with *all* observations and no theory that has "perfect" simplicity. Therefore, in every individual case, one has to make a choice of a theory by a compromise between both criterions. However, when we try to specify the degree of "simplicity" in different theories, we soon notice that attempts of this kind lead us far beyond the limits of physical science. Everybody would agree that a linear function is simpler than a function of the second or higher degree; everybody would also admit that a circle is simpler than an ellipse. For this reason, physics is filled with laws that express proportionality, such as Hooke's law in elasticity or Ohm's law in electrodynamics. In all these cases, there is no doubt that a nonlinear relationship

would describe the facts in a more accurate way, but one tries to get along with a linear law as much as possible.

There was a time when, in physics, laws that could be expressed without using differential calculus were preferred, and in the long struggle between the corpuscular and the wave theories of light, the argument was rife that the corpuscular theory was mathematically simpler, while the wave theory required the solution of boundary problems of partial differential equations, a highly complex matter. We note that even a purely mathematical estimation of simplicity depends upon the state of culture of a certain period. People who have grown up in a mathematical atmosphere—that is, saturated with ideas about invariants—will find that Einstein's theory of gravitation is of incredible beauty and simplicity; but to people for whom ordinary calculus is the center of interest, Einstein's theory will be of immense complexity, and this low degree of simplicity will not be compensated by a great number of observed facts.

However, the situation becomes much more complex, if we mean by *simplicity* not only simplicity of the mathematical scheme but also simplicity of the whole discourse by which the theory is formulated. We may start from the most familiar instance, the decision between the Copernican (heliocentric) and the Ptolemaic (geocentric) theories. Both parties, the Roman Church and the followers of Copernicus, agreed that Copernicus' system, from the purely mathematical angle, was simpler than Ptolemy's. In the first one, the orbits of planets were plotted as a system of concentric circles with the sun as center, whereas in the geocentric system, the planetary orbits were sequences of loops. The observed facts covered by these systems were approximately the same ones. The criterions of acceptance that are applied in the community of scientists today are, according to the usual way of speaking, in agreement with observed facts and mathematical simplicity. According to them, the Copernican system had to be accepted unhesitatingly. Since this acceptance did not happen before a long period of doubt, we see clearly that the criterions "agreement with observed facts" and "mathematical simplicity" were not the only criterions that were considered as reasons for the acceptance of a theory.

As a matter of fact, there were three types of reasons against the acceptance of the Copernican theory that remained unchallenged at the time when all "scientific" reasons were in favor of that theory. First, there was the incompatibility of the Copernican system with the traditional interpretation of the Bible. Second, there was the disagreement between the Copernican system and the prevailing philosophy of that period, the philosophy of Aristotle as it was interpreted by the Catholic Schoolmen. Third, there was the objection that the mobility of the earth, as a real physical fact, is incompatible with the common-sense interpretation of nature. Let us consider these three types of reasons more closely. In the Book of Joshua this leader prays to God to stop the sun in its motion in order to prolong the day and to enable the people of Israel to win a decisive victory. God indeed "stopped the sun." If interpreted verbally, according to the usage of words in our daily language, this means that the sun is moving, in flagrant contradiction with the Copernican theory. One could, of course, give a more sophisticated interpretation and say that "God stopped the sun" means that he stopped it in its motion relative to the earth. This is no longer contradictory to the Copernican system. But now the question arises: Should we adopt a simple mathematical description and a complicated, rather "unnatural" interpretation of the Bible or a more complicated mathematical description (motion in loops) and a simple "natural" interpretation of the biblical text? The decision certainly cannot be achieved by any argument taken from physical science.

If one believes that all questions raised by science must be solved by the "methods" of this special science, one must say: Every astronomer who lived in the period between Copernicus and Galileo was "free" to accept either the Copernican or the Ptolemaic doctrine; he could make an "arbitrary" decision. However, the situation is quite different if one takes into consideration that physical science is only a part of science in general. Building up astronomical theories is a particular act of human behavior. If we consider human behavior in general, we look at physical science as a part of a much more general endeavor that embraces also psychology and sociology. It is called by some authors "behavioristics." From this more general angle, the effect of a simplification in the mathematical formula and the simplification in biblical interpretation are quite comparable with each other. There is meaning in asking by which act the happiness of human individuals and groups is more favorably influenced. This means that, from the viewpoint of a general science of human behavior, the decision between the Copernican and Ptolemaic systems was never a matter of arbitrary decision.

The compatibility of a physical theory with a certain interpretation of the Bible is a special case

REASONS FOR THE ACCEPTANCE OF SCIENTIFIC THEORIES

These three articles and subsequent discussion—by Philipp G. Frank, C. West Churchman, Barrington Moore, Jr., and Richard Rudner—comprised a symposium on "Reasons for the acceptance of scientific theories" held in Boston, Mass., on 27 Dec. 1953, as a joint session of AAAS Section L, the Philosophy of Science Association, and the Institute for the Unity of Science; acting as cosponsors were the National Science Foundation and the American Academy of Arts and Sciences. This symposium was the first of five sessions held in Boston, 27–30 Dec., on the general subject "Validation of Scientific Theories." The remaining sessions were sponsored by the American Academy of Arts and Sciences, the Institute for the Unity of Science, and the National Science Foundation. They were devoted, respectively, to "The present state of operationalism," "Psychoanalysis and scientific method," "Organism and machine," and "Science as a social and historical phenomenon." The papers presented in these sessions will appear, one group at a time, in subsequent issues.

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of a much more general criterion: the compatibility of a physical theory with theories that have been advanced to account for observable phenomena outside the domain of physical science. The most important reason for the acceptance of a theory beyond the "scientific criterions" in the narrower sense (agreement with observation and simplicity of the mathematical pattern) is the fitness of a theory to be generalized, to be the basis of a new theory that does not logically follow from the original one, and to allow prediction of more observable facts. This property is often called the "dynamical" character or the "fertility" of a theory. In this sense, the Copernican theory is much superior to the geocentric one. Newtonian laws of motion have a simple form only if the sun is taken as a system of reference and not the earth. But the decision in favor of the Copernican theory on this basis could be made only when Newton's laws came into existence. This act requires, however, creative imagination or, to speak more flippantly, a happy guessing that leads far beyond the Copernican and Ptolemaic systems.

However, long before the "dynamical" character of the Copernican system was recognized, the objection was raised that the system was incompatible with "the general laws of motion" that could be derived from principles regarded as "immediately intelligible" or, in other words, "self-evident" without physical experiment or observations. From such "self-evident" principles there followed, for example, the physical law that only celestial bodies (like sun or moon) moved "naturally" in circular orbits, while terrestrial bodies (like our earth) moved naturally along straight lines as a falling stone does. Copernicus' theory of a "motion of the earth in a circular orbit" was, therefore, incompatible with "self-evident" laws of nature.

Medieval scientists were faced with the alternatives: Should they accept the Copernican theory with its simple mathematical formulas and drop the self-evident laws of motion, or should they accept the complicated mathematics of the Ptolemaic system along with the intelligible and self-evident general laws of motion. Acceptance of Copernicus' theory would imply dropping the laws of motion that had been regarded as self-evident and looking for radically new laws. This would also mean dropping the contention that a physical law can be derived from "intelligible" principles. Again, from the viewpoint of physical science, this decision cannot be made. Although an arbitrary decision may seem to be required, if one looks at the situation from the viewpoint of human behavior it is clear that the decision, by which the derivation of physical laws from self-evident principles is abandoned, would alter the situation of man in the world fundamentally. For example, an important argument for the existence of spiritual beings would lose its validity. Thus, social science had to decide whether the life of man would become happier or unhappier by the acceptance of the Copernican system.

The objections to this system, on the basis of self-evident principles, have also been formulated in a way that looks quite different but may eventually, when the chips are down, not be so very different. Francis Bacon, the most conspicuous adversary of Aristotelianism in the period of Galileo, fought the acceptance of the Copernican theory on the basis of common-sense experience. He took it for granted that the principles of science should be as analogous as possible to the experience of our daily life. Then, the principles could be presented in the language that has been shaped for the purpose of describing, in the most convenient way, the experience of our daily existence—the language that everyone has learned as a child and that is called "common-sense language." From this daily experience, we have learned that the behavior of the sun and the planets is very different from that of the earth. While the earth does not emit any light, the sun and the planets are brilliant; while every earthly object that

becomes separated from the main body will tend to fall back toward the center and stop there, the celestial objects undergo circular motion eternally around the center.

To separate the sun from the company of the planets and put the earth among these brilliant and mobile creatures, as Copernicus suggested, would have been not only unnatural but a serious violation of the rule to keep the principles of science as close to common sense as possible. We see by this example that one of the reasons for the acceptance of a theory has frequently been the compatibility of this theory with daily life experience or, in other words, the possibility of expressing the theory in common-sense language. Here is, of course, the source of another conflict between the "scientific" reasons for the acceptance of a theory and other requirements that are not "scientific" in the narrower sense. Francis Bacon rejected the Copernican system because it was not compatible with common sense.

In the 18th and 19th centuries, Newton's mechanics not only had become compatible with common sense but had even been identified with common-sense judgment. As a result, in 20th century physics, the theory of relativity and the quantum theory were regarded by many as incompatible with common sense. These theories were regarded as "absurd" or, at least, "unnatural." Lenard in Germany, Bouasse in France, O'Rahilly in Ireland, and Timiryaseff in Russia rejected the theory of relativity, as Francis Bacon had rejected the Copernican system. Looking at the historical record, we notice that the requirement of compatibility with common sense and the rejection of "unnatural theories" have been advocated with a highly emotional undertone, and it is reasonable to raise the question: What was the source of heat in those fights against new and absurd theories? Surveying these battles, we easily find one common feature, the apprehension that a disagreement with common sense may deprive scientific theories of their value as incentives for a desirable human behavior. In other words, by becoming incompatible with common sense, scientific theories lose their fitness to support desirable attitudes in the domain of ethics, politics, and religion.

Examples are abundant from all periods of theory-building. According to an old theory that was prevalent in ancient Greece and was accepted by such men as Plato and Aristotle, the sun, planets, and other celestial bodies were made of a material that was completely different from the material of which our earth consists. The great gap between the celestial and the terrestrial bodies was regarded as required by our common-sense experience. There were men—for example, the followers of Epicurus—who rejected this view and assumed that all bodies in the universe, earth and stars, consist of the same material. Nevertheless, many educators and political leaders were afraid that denial of the exceptional status of the celestial bodies in physical science would make it more difficult to teach the belief in the existence of spiritual beings as distinct from material bodies; and since it was their general conviction that the belief in spiritual beings is a powerful instrument to bring about a desirable conduct among citizens, a physical theory that supported this belief seemed to be highly desirable.

Plato, in his famous book *Laws*, suggested that people in his ideal state who taught the "materialistic" doctrine about the constitution of sun and stars should be jailed. He even suggested that people who knew about teachers of that theory and did not report them to the authorities should also be jailed. We learn from this ancient example how scientific theories have served as instruments of indoctrination. Obviously, fitness to support a desirable conduct of citizens or, briefly, to support moral behavior, has served through the ages as a reason for acceptance of a theory. When the "scientific criterions" did not uniquely determine a theory, its fitness to support moral or political indoctrination became an important factor for its acceptance. It is important to learn that the interpretation of a scientific theory as a support of moral rules is not a rare case but has played a role in all periods of history.

This role probably can be traced back to a fact that is well known to modern students of anthropology and sociology. The conduct of man has always been shaped according to the example of an ideal society; on the other hand, this ideal has been represented by the "behavior" of the universe, which is, in turn, determined by the laws of nature, in particular, by the physical laws. In this sense, the physical laws have always been interpreted as examples for the conduct of man or, briefly speaking, as moral laws. Ralph Waldo Emerson wrote in his essay *Nature* that "the laws of physics are also the laws of ethics." He used as an example the law of the lever, according to which "the smallest weight may be made to lift the greatest, the difference of weight being compensated by time."

We see the connection of the laws of desirable human conduct with the physical laws of the universe when we glance at the Book of Genesis. The first chapter presents a physical theory about the creation of the world. But the story of the creation

serves also as an example for the moral behavior of man; for instance, because the creation took 7 days, we all feel obliged to rest on each seventh day. Perhaps the history of the Great Flood is even more instructive. When the Flood abated, God established a Covenant with the human race: "Never again shall all flesh be cut off by the waters of a flood and destroy the earth." As a sign of the Covenant the rainbow appeared: "When I bring clouds over the earth and the bow is seen in the clouds, I will remember the Covenant which is between me and you, and the waters shall never again become a flood to destroy all flesh." If we read the biblical text carefully, we understand that what God actually pledged was to maintain, without exception, the validity of the physical laws or, in other words, of the causal law. God pledged: "While the earth remains, seedtime and harvest, cold and heat, summer and winter, day and night shall not cease."

All the physical laws, including the law of causality, were given to mankind as a reward for moral behavior and can be canceled if mankind does not behave well. So even the belief in the validity of causal laws in the physical world has supported the belief in God as the supreme moral authority who would punish every departure from moral behavior by abolishing causality. We have seen that Epicurean physics and Copernican astronomy were rejected on moral grounds. We know that Newton's physics was accepted as supporting the belief in a God who was an extremely able engineer and who created the world as a machine that performed its motions according to His plans. Even the generalization of Newtonian science that was advanced by 18th century materialism claimed to serve as a support for the moral behavior of man. In his famous book *Man a Machine*, which has often been called an "infamous book," La Mettrie stresses the point that by regarding men, animals, and planets as beings of the same kind, man is taught to regard them all as his brothers and to treat them kindly.

It would be a great mistake to believe that this situation has changed in the 19th and 20th centuries. A great many authors have rejected the biological theory that organisms have arisen from inanimate matter (spontaneous generation), because such a theory would weaken the belief in the dignity of man and in the existence of a soul and would, therefore, be harmful to moral conduct. In 20th century physics, we have observed that Einstein's theory of relativity has been interpreted as advocating an "idealistic" philosophy, which, in turn, would be useful as a support of moral conduct. Similarly, the quantum theory is interpreted as supporting a weakening of mechanical determinism and, along with it, the introduction of "indeterminism" into physics. In turn, a great many educators, theologians, and politicians have enthusiastically acclaimed this "new physics" as providing a strong argument for the acceptance of "indeterminism" as a basic principle of science.

The special mechanism by which social powers bring about a tendency to accept or reject a certain theory depends upon the structure of the society within which the scientist operates. It may vary from a mild influence on the scientist by friendly reviews in political or educational dailies to promotion of his book as a best seller, to ostracism as an author and as a person, to loss of his job, or, under some social circumstances, even to imprisonment, torture, and execution. The honest scientist who works hard in his laboratory or computation-room would obviously be inclined to say that all this is nonsense—that his energy should be directed toward finding out whether, say, a certain theory is "true" and that he "should not" pay any attention to the fitness of a theory to serve as an instrument in the fight for educational or political goals. This is certainly the way in which the situation presents itself to most active scientists. However, scientists are also human beings and are definitely inclined toward some moral, religious, or political creed. Those who deny emphatically that there is any connection between scientific theories and religious or political creeds believe in these creeds on the basis of indoctrination that has been provided by organizations such as churches or political parties. This attitude leads to the conception of a "double truth" that is not only logically confusing but morally dangerous. It can lead to the practice of serving God on Sunday and the Devil on weekdays.

The conviction that science is independent of all moral and political influences arises when we regard science either as a collection of facts or as a picture of objective reality. But today, everyone who has attentively studied the logic of science will know that science actually is an instrument that serves the purpose of connecting present events with future events and deliberately utilizes this knowledge to shape future physical events as they are desired. This instrument consists of a system of propositions—principles—and the operational definitions of their terms. These propositions certainly can not be derived from the facts of our experience and are not uniquely determined by these facts. Rather they are hypotheses from which the facts can be logically derived. If the principles or hypotheses are not determined by the physical facts, by what are they determined? We have learned by now that, besides

the agreement with observed facts, there are other reasons for the acceptance of a theory: simplicity, agreement with common sense, fitness for supporting a desirable human conduct, and so forth. All these factors participate in the making of a scientific theory. We remember, however, that according to the opinion of the majority of active scientists, these extrascientific factors "should not" have any influence on the acceptance of a scientific theory. But who has claimed and who can claim that they "should not"?

This firm conviction of the scientists comes from the philosophy that they have absorbed since their early childhood. The theories that are built up by "scientific" methods, in the narrower sense, are "pictures" of physical reality. Presumably they tell us the "truth" about the world. If a theory built up exclusively on the ground of its agreement with observable facts tells the "truth" about the world, it would be nonsense to assume seriously that a scientific theory can be influenced by moral or political reasons. However, we learned that "agreement with observed facts" does not single out one individual theory. We never have one theory that is in full agreement but several theories that are in partial agreement, and we have to determine the final theory by a compromise. The final theory has to be in fair agreement with observations and also has to be sufficiently simple to be usable. If we consider this point, it is obvious that such a theory cannot be "the truth." In modern science, a theory is regarded as an instrument that serves toward some definite purpose. It has to be helpful in predicting future observable facts on the basis of facts that have been observed in the past and the present. It should also be helpful in the construction of machines and devices that can save us time and labor. A scientific theory is, in a sense, a tool that produces other tools according to a practical scheme.

In the same way that we enjoy the beauty and elegance of an airplane, we also enjoy the "elegance" of the theory that makes the construction of the plane possible. In speaking about any actual machine, it is meaningless to ask whether the machine is "true" in the sense of its being "perfect." We can ask only whether it is "good" or sufficiently "perfect" for a certain purpose. If we require speed as our purpose, the "perfect" airplane will differ from one that is "perfect" for the purpose of endurance. The result will be different again if we choose safety, or fun, or convenience for reading and sleeping as our purpose. It is impossible to design an airplane that fulfills all these purposes in a maximal way. We have to make some compromises. But then, there is the question: Which is more important, speed or safety, or fun or endurance? These questions cannot be answered by any agreement taken from physical science. From the viewpoint of "science proper" the purpose is arbitrary, and science can teach us only how to construct a plane that will achieve a specified speed with a specified degree of safety. There will be a debate, according to moral, political, and even religious lines, by which it will be determined how to produce the compromise. The policy-making authorities are, from the logical viewpoint, "free" to make their choice of which type of plane should be put into production. However, if we look at the situation from the viewpoint of a unified science that includes both physical and social science, we shall understand how the compromise between speed and safety, between fun and endurance is determined by the social conditions that produce the conditioned reflexes of the policy makers. The conditioning may be achieved, for example, by letters written to Congressmen. As a matter of fact, the building of a scientific theory is not essentially different from the building of an airplane.

If we look for an answer to the question of whether a certain theory, say the Copernican system or the theory of relativity, is perfect or true, we have to ask the preliminary questions: What purpose is the theory to serve? Is it only the purely technical purpose of predicting observable facts? Or is it to obtain a simple and elegant theory that allows us to derive a great many facts from simple principles? We choose the theory according to our purpose. For some groups, the main purpose of a theory may be to serve as a support in teaching a desirable way of life or to discourage an undesirable way of life. Then, we would prefer theories that give a rather clumsy picture of observed facts, provided that we can get from the theory a broad view of the universe in which man plays the role that we desire to give him. If we wish to speak in a more brief and general way, we may distinguish just two purposes of a theory: the usage for the construction of devices (technological purpose) and the usage for guiding human conduct (sociological purpose).

The actual acceptance of theories by man has always been a compromise between the technological and the sociological usage of science. Human conduct has been influenced directly by the latter, by supporting specific religious or political creeds, while the technological influence has been rather indirect. Technological changes have to produce social changes that manifest themselves in changing human conduct. Everybody knows of the Industrial Revolution of the 19th century and the accompanying changes in human life from a rural

into an urban pattern. Probably the rise of atomic power will produce analogous changes in man's way of life.

The conflict between the technological and the sociological aims of science is the central factor in the history of science as a human enterprise. If thoroughly investigated, it will throw light upon a factor that some thinkers, Marxist as well as religious thinkers, regard as responsible for the social crisis of our time: the backwardness of social progress compared with technological progress. To cure this illness of our time, a British bishop recommended, some years ago, the establishment of a "truce" in the advancement in technology, in order to give social progress some time to keep up with technological advancement. We have seen examples of this conflict in Plato's indictment of astrophysical theories that could be used as a support of "materialism." We note the same purpose in the fight against the Copernican system and, in our own century, against the Darwinian theory of evolution, against Mendel's laws of heredity, and so forth.

A great many scientists and educators believe that such a conflict no longer exists in our time, because now it is completely resolved by "the scientific method" which theory is the only valid one. This opinion is certainly wrong if we consider theories of high generality. In 20th century physics, we note clearly that a formulation of the general principles of subatomic physics (quantum theory) is accepted or rejected according to whether we believe that introduction of "indeterminism" into physics gives comfort to desirable ethical postulates or not. Some educators and politicians have been firmly convinced that the belief in "free will" is necessary for ethics and that "free will" is not compatible with Newtonian physics but is compatible with quantum physics. The situation in biology is similar. If we consider the attitude of biologists toward the question of whether living organisms have developed from inanimate matter, we shall find that the conflict between the technological and the sociological purposes of theories is in full bloom. Some prominent biologists say that the existence of "spontaneous generation" is highly probable, while others of equal prominence claim that it is highly improbable. If we investigate the reasons for these conflicting attitudes, we shall easily discover that, for one group of scientists, a theory that claims the origin of man not merely from the "apes" but also from "dead matter" undermines their belief in the dignity of man, which is the indispensable basis of all human morality. We would note in turn that, for another group, desirable human behavior is based on the belief that there is a unity in nature that embraces all things.

In truth, many scientists would say that scientific theories "should" be based only on purely scientific grounds. But, exactly speaking, what does the word *should* mean in this context? With all the preceding arguments it can mean only: if we consider exclusively the technological purpose of scientific theories, we could exclude all criterions such as agreement with common sense or fitness for supporting desirable conduct. But even if we have firmly decided to do away with all "nonsense," there still remains the criterion of "simplicity," which is necessary for technological purposes and also contains, as we learned previously, a certain sociological judgment. Here, restriction to the purely technological purpose does not actually lead unambiguously to a scientific theory. The only way to include theory-building in the general science of human behavior is to refrain from ordering around scientists by telling them what they "should" do and to find how each special purpose can be achieved by a theory. Only in this way can science as a human activity be "scientifically" understood and the gap between the scientific and the humanistic aspect be bridged.

Institute for the Unity of Science

The Institute for the Unity of Science is based on the ideas of the International Unity of Science movement, which has had meetings in Paris (1935), Copenhagen (1936), Cambridge, England (1938), and Cambridge, Massachusetts (1939). The Institute was founded in 1947 and has been supported by the Rockefeller Foundation and the American Academy of Arts and Sciences. Its purpose is to work for cooperation between the sciences and between the sciences and the humanities and against the isolated position of philosophy as a separate department by which it has become a new special field instead of being the center for a unified look on human knowledge and human action.

The Institute publishes the *Encyclopedia for Unified Science* and *Contributions to the Analysis and Synthesis of Knowledge*. There have been frequent meetings in the Boston area and several national meetings on both the East coast and the West coast. The topic of these meetings was the study of the general structure of human knowledge in all fields. The goal is to analyze not only the logic of science but also the psychological and sociological factors pertinent in the evolution of knowledge. It has become more and more clear that the belief in the validity of a scientific theory is based on logical arguments as well as on arguments taken from psychological and sociological research.—PHILIPP G. FRANK.

Influence of Political Creeds on the Acceptance of Theories

BARRINGTON MOORE, JR.

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FEW people today are likely to argue that the acceptance of scientific theories, even by scientists themselves, depends entirely upon the logical evidence adduced in support of these theories. Extraneous factors related to the philosophical climate and society in which the scientist lives always play at least some part. The interesting problem, therefore, becomes not one of ascertaining the existence of such factors but one of appraising the extent of their impact under different conditions. My task here (*1*) is confined to interpreting a few high lights of the evidence I have been able to gather on this point for the Soviet Union. A much fuller discussion is available in a recently published general study of the sources of stability and change in the Soviet dictatorship (*2*).

Since others in this symposium discuss the content of the official orthodoxy, dialectical materialism, there is no need for me to deal with this topic in any detail. I should like, however, to draw attention to two features. One is that the doctrine has a strong bias against formal and abstract thinking, such as that found in pure mathematics, symbolic logic, and much of modern physics. The other is this: We all know that the doctrine pretends to be a universal and cosmic explanation of all natural and human phenomena. Yet, as a practical matter, the areas where it is applied and the ways in which it is interpreted vary considerably with changing political circumstances. There is, therefore, a fluctuating and ill-defined boundary, a sort of no man's land, between the area of politically determined truth and the area of scientifically determined truth. From about the end of World War II until Stalin's death, the area of political truth expanded steadily, not only in genetics, but also in physics, chemistry, and mathematics. Since Stalin's death there have been numerous signs of a retreat on the intellectual front. There have not, however, been any significant changes in the control machinery itself.

There are two main ways in which the Soviet system attempts to bring about the political orthodoxy of science. One way is through giving the more distinguished scientists higher incomes and greater material comforts than the rest of the population. This high social and economic status is held upon the condition of outward political conformity. The other method is administrative. Soviet science is highly bureaucratized. The independent scientist working on his own and freely choosing his own problems does not exist in the U.S.S.R. Instead, he must find his niche in a rambling bureaucratic structure. This state of affairs is, of course, by no means wholly attributable to the totalitarian aspects of the Soviet system. Probably it is a product of changes brought about by the development of industrial society generally. Indeed, the easiest way for an American to capture the atmosphere of Soviet science would be to recall our large government research projects, although there are significant distinctions between the two situations.

It is unnecessary to present here a detailed analysis of the administrative apparatus in Soviet science. Essentially, it is composed of three elements, the Communist Party, the secret police, and the technically qualified scientist, a triumvirate that has its counterpart in every Soviet organization from a scientific laboratory to a factory, an army unit, a sports club, or a collective farm. A few comments on the ways in which this control system does and does not affect actual scientific work may be of interest.

The aspect of Soviet science that is perhaps most puzzling to us in the Western world is the notion

of planning. In actual practice, the plan or research program of any given laboratory often represents the outcome of a tug of war between the desires of the political leaders and the wishes of the scientists themselves. Despite numerous efforts to tighten controls after the war, there are clear indications that the natural scientists managed to retain a good deal of autonomy, or at least much more autonomy than the rulers would like. For example, in Oct. 1952, a high Party official from Leningrad, Moscow's rival as the chief center of scientific activity in the U.S.S.R., observed sarcastically in a public speech that the hand of the State Planning Commission could not be felt in his city's scientific research establishments.

In general, it appears that the forms of direct control, such as planning and probably even the continuous barrage of Marxist-Leninist propaganda, have less effect on the content of Soviet science and, hence, on what the Soviet scientist accepts as valid theory than do somewhat more subtle factors. In this connection, I hasten to add that I am not a natural scientist myself and that it would require someone with both sociological skills and the appropriate natural science skills to confirm or disprove this tentative judgment. But one may point to certain factors connected with the career expectation of Soviet scientists as major elements in influencing the choice that they make among various bodies of thought. The most effective power of the Communist regime lies, I think, in deciding which kinds of research will be rewarded upon completion and which kinds of research will in effect be penalized. The immediate point of impact is on the career expectations of advanced students as well as on mature scientists who want to progress up the ladder of rank and prestige. Since some calculation of these factors must be made on a "realistic" basis by even the most idealistic or the most strictly logical scientist, the force of these controls extends backward to an early stage in the scientist's career. It is especially important at the time the scientist selects his field of study. However, it is only when a student reaches the closing period of his university studies that he knows whether or not he has placed his chips wisely.

The major elements in the situation are the following. The Ministry of Culture is responsible for confirming appointments to research and teaching staffs and for granting higher degrees, although the initial recommendations are usually made by qualified scientists and scholars. In 1951, the Ministry refused to grant 10.7 percent of the doctoral degrees that had been recommended during the preceding 3 years. No indication is available as to how many of these refusals were based on strictly political grounds. But the reports of the Ministry, which receive wide publicity in the central newspapers, usually select several cases of refusal on political grounds to serve as warnings to Soviet youth. Thus, in 1948, the authorities refused to confer the title of professor upon a doctor of physicomathematical sciences, V. L. Ginzburg, on the grounds that he had, even in his popular works, circumvented the achievements of Soviet science and displayed obvious servility toward foreign achievements.

There is also evidence that students, in calculating their chances for a career, tend to choose either a subject in which the Party has laid down a clear and definite line or one about which the Party has expressed no opinion. In other words, they choose either the areas where political truths are definite or where scientific truths can be more or less freely ascertained. They try to steer clear of dangerous border areas. Thus, students avoided writing dissertations on genetics for quite some time before the Party made Lysenko's views an official orthodoxy, a decision that was not reached until a series of minor forays had been made by the Party into this area. Three years before the Lysenko decision, a high Soviet official complained that dissertations on genetics were becoming so rare as to be almost unique events. But in history and philosophy, on which the Party made a series of clear-cut pronouncements after the war, there was a sixfold rise in the number of dissertations, while the total increase in all fields during this period amounted to only 34.4 percent.

Since the Soviets place a very high value on certain kinds of scientific research and technical training, the Party and the police do not ordinarily interfere in the routine activities of the scientist. In fields where the Party has not issued a pronouncement, the scientist can set his own standards for governing the appraisal of evidence and reasoning in research and can set corresponding standards for the qualifications of his students. By the late 1930's, it became possible for a teacher to fail a Komsomol, or Party member, who was clearly incompetent, although the teacher might have an anxious moment in the process. It appears that this situation prevails widely in many areas of research in the natural sciences at any given moment. The propagation of Marxism-Leninism tends to be reduced to a formality, accepted as a boring necessity by all concerned. In this fashion, the rulers reach a compromise with the autonomous and distinctive requirements of scientific activity.

In the latter years of Stalin's regime, however,

this compromise was at least partly upset in several fields. The discussion of genetics in the summer of 1948 is the incident that has attracted the most attention in the West. This event was merely part of a much larger movement. Philosophy, biology, linguistics, physiology, cosmogony, chemistry, physics, and mathematics have all, in widely varying degrees, felt the impact of this movement. In June 1951, to cite merely one example, the Academy of Sciences itself was forced to put the stamp of disapproval on the use of quantum theory, mainly because of its supposed connection with "decadent" Western idealism and formalist abstraction. Although Soviet scientists undoubtedly continue to use quantum theory in their actual work, uncertainty about where the political lightning may strike next must be a vital element in the over-all situation confronting them. The partial relaxation that has followed Stalin's death can scarcely have removed this anxiety. Soviet scientists with any memory of the past are aware that previous periods of relaxation have been followed by new and stricter orthodoxies, and they will remain on their guard.

The examination of the Soviet case prompts some general reflections on the influence of political creeds in science. A totalitarian creed that claims to encompass all human affairs in one sweep is bound to come into conflict with scientific theory at some point. The essence of a totalitarian system is that it tries to impose some single criterion of appropriate behavior on every aspect of human action, from the choice of a marriage partner to the choice of scientific beliefs. Naturally it does not succeed entirely, and it has to come to terms with the autonomous requirements of these activities. As long as a totalitarian regime seeks the benefits of science, it must make some compromise with the scientists' own methods of reaching and determining truth. The compromise may be made easier where the rationalist ethic, that which is part and parcel of the rise of our industrial civilization, has destroyed any sense of moral and ethical commitment among scientists. Then scientists may be quite willing to accept political direction from any source.

A democratic creed, on the other hand, does not claim to present answers to all possible questions or to pose the way in which all human activities must be carried out. The way it attempts to solve the problem of reaching some kind of harmony in all the affairs of society, including scientific affairs, is a different one and provides greater flexibility. Up to a point, the democratic creed provides for its own modification through established and agreed-upon rules. Such modifications can come from science as well as from other sources.

I would suggest, however, that no society, not even a democratic one, can accept science as the sole method and source for the modification of its established creed. For reasons that cannot be given here in any detail, it appears likely that any large human society requires a set of beliefs about the purposes of life and the ways in which it is legitimate and not legitimate to achieve these purposes. This set of beliefs constitutes the political truths of the society. Since they involve judgments of value, I do not think that they can ever be completely amenable to rational discussion. Therefore, as science develops its own canons for validating its propositions, there is likely to come a time when the political creed and the scientific creed conflict with each other. I am inclined to believe that some element of conflict will usually be present, despite the contemporary efforts of some religious and scientific authorities to make polite bows in each other's direction. Methods of control and compromise will then have to be found. At any rate, the conflict is a familiar one in the history of Western culture.

In other words, the totalitarian case, including that of the U.S.S.R., merely constitutes an extreme one. The situation there does not differ from our own in any absolute sense. Nor can all the features of the Soviet scene that many of us find repugnant be traced entirely to the totalitarian bacillus. In the growth of organized research, the emphasis on practical results, and the stress on political conformity, one can perceive in the Soviet situation elements that represent, as it were, a horrible image in a distorted mirror of our own possible future. Major similarities can be traced to the fact that both the United States and the Soviet Union are industrial societies.

Fortunately, the situation in Russia is only a possible image of our future and is by no means an inevitable one. As E. H. Carr has remarked, history does not repeat itself because the actors on the stage already know the outcome of the previous act (3). The experience of others, to the extent that we understand it and use it, becomes part of our own situation and helps us to make it different. Let us hope that fuller understanding of Soviet science may help the West to avoid a similar fate.

References and Notes

1. I wish to acknowledge my debt to the Russian Research Center of Harvard University, under whose auspices this work was done.
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3. *The New Society* (London, 1951), p. 6.

Notes on a Pragmatic Theory of Induction

C. WEST CHURCHMAN

Dr. Churchman received training at the University of Pennsylvania and taught philosophy at Wayne University and the University of Pennsylvania, where he headed the department. From 1942–45, he was in charge of the mathematics section of the Frankford Arsenal Ordnance Laboratory. Dr. Churchman is the author of several books on measurement of consumer interest, the theory of experimental inference and methods of making experimental inferences, methods of inquiry, and psychologistics. He has also written papers and reviews on philosophy of science, mathematical statistics, logic, and operations research. At present Dr. Churchman is in charge of the Operations Research Group of the Engineering Administration Department at Case Institute of Technology, Cleveland.

THE purpose here is to outline the problem of induction when this problem is phrased in pragmatic terms—that is, when the problem is phrased in terms of the relationship between evidence and decisions. Pragmatism is presumably interested in the relationship of actions to actions and not simply in the relationship of "sentences" to "sentences." Hence, the pragmatic problem of induction must be stated in a language of human actions—that is, in the language of the social sciences.

The following terms appear to be critical: *evidence gathering,* which is a reaction of an individual to the environment, when such reaction is considered from the viewpoint of an increase in knowledge of the individual relative to a set of objectives; *decision,* which is a potential action of an individual, when such action is considered from the viewpoint of its efficiency for accomplishing one or more objectives; *efficiency,* which is the probability (or some analogous measure) that a decision will produce an outcome; *objective,* which is a state of nature, or part of a state of nature, potentially producible by a decision; *value,* which is a measure associated with each objective; *policy* (strategy), which is a rule for the selection of a decision out of a set of decisions at a moment of time; this rule may be such as to determine uniquely the decision, or the rule may specify a random device (flipping a coin) for the selection; *increase in knowledge,* which is a measure of the efficiency of a policy over a period of time relative to a set of objectives and their values.

These definitions are intentionally interdependent (circular), based on the methodological supposition that definitions are not analytic (reductions to simples) but rather interconceptual (exhibitions of the interrelationships of concepts).

The pragmatic problem of induction is the determination of the optimum relationship between evidence gathering and the determination of policies. The problem includes not only the relation of *given* evidence to policy formation but also the relation of the policies of gathering evidence to other types of policies (for example, the sampling theory and experimental design theories are evidence-gathering policies).

The pragmatic problem of induction depends, among other things, on a determination of the efficiency of policies. Some policies are considered "rational," some are not. Can this concept be defined and measured? This paper exhibits the types of problems that arise in connection with a definition of policy efficiency. Problems may be classified into the following types.

1) *Deterministic one-aim problems with certainty,* where (i) decisions are judged in terms of one objective; (ii) there exists a finite set of evidence sufficient to determine the efficiency of a decision for the objective: (iii) every decision has either a perfect chance of success or a zero chance. Examples are puzzles, simple games, and some mathematical derivations. Here it seems clear that there is only one "perfect" policy consisting of the decision to gather the minimum evidence set necessary to guarantee a perfect decision leading to the objective. The effectiveness of other policies may be measured in terms of the additional "surplus"

evidence gathering necessary to find the perfect solution.

2) *Statistical one-aim problems with certainty,* where (i) decisions are judged in terms of one objective; (ii) there exists a finite set of evidence (virtually) sufficient to determine the efficiency of a decision for the objective; (iii) every decision has a definite probability of attaining the objective. Simple gambling games are examples. The efficiency of policies can be measured in terms of the "surplus" of evidence gathering plus the efficiency of the decision that is chosen, although the exact form of this measurement may not always be clear.

3) *One-aim problems with double uncertainty,* where (i) one objective is to be served; (ii) each decision has a definite probability of success relative to the objective; (iii) there exists no finite set of evidence sufficient to determine the possibilities of success of the decisions; (iv) estimates of the probabilities can be made, and estimates of the errors of these estimates can be made. Examples are simple scientific theories, where "curiosity-satisfaction" is the objective, or a company's policy, where so-called net return or profit, is the only objective. Such problems are "doubly uncertain," because the decisions are not certain to produce the desired result, and because the "uncertainty" can only be estimated within error limits.

Here the measures of efficiency of a policy become less easily definable. One might argue for the policy that leads to the maximum *estimated* probability of success. This intuitive guess, however, would in some cases be incorrect, for the errors of the estimates might be quite critical. For example, let us suppose that one decision is estimated to have a probability of success of 0.75 but that the error of this estimate is 0.25. That is to say, the *true* probability may lie anywhere between 0.5 and 1.0. The second decision is estimated to have a probability of success of 0.70, but this estimate is accurate within 0.01. That is, the true value should lie between 0.69 and 0.71. A policy that selects the first decision is not clearly the best policy. It would be the best policy only if one believed that nature "is either beneficent or indifferent." The choice of the second decision may be a far better one, especially if one believes that nature is inclined to be an opponent.

In this case then, the effectiveness of a policy is to be measured by some function of the evidence-gathering effort, the estimated probabilities of the success of the decisions, and the errors of these estimates. The form of this function is not "clear."

4) *Multiaim problems with double uncertainty,* where (i) more than one objective is to be served by the policy; (ii) no decision has maximum effectiveness for all the objectives; (iii) a definite probability of success exists for each decision relative to each objective; (iv) there exists no finite set of evidence sufficient to guarantee the probabilities of success; (v) the probabilities of success can be estimated, and the errors of these estimates can also be estimated; (vi) the values of the objectives are known or assumed without error. Examples are policy problems of agencies, industries, and so forth, where the goals are accepted as definitive. Here policy is judged in terms of the evidence-gathering effort, the estimated probabilities of success of the decisions relative to each objective, the errors of these estimates, and the "given" values of the objectives. The proper form of this function becomes even less "clear."

5) *Multiaim problems with triple uncertainty,* where (i) more than one objective is to be served; (ii) no decision has maximum effectiveness for all objectives; (iii) a definite probability of success exists for each decision relative to each objective; (iv) there exists no finite set of evidence sufficient to guarantee the probabilities of success; (v) the probabilities of success can be estimated, and the errors of these estimates can be estimated; (vi) there exists no finite set of evidence sufficient to guarantee the values of the objectives; (vii) the values of the objectives can be estimated, and the errors of these estimates can be estimated. Examples are all scientific, community, and industrial problems. The evaluation of policies in this last problem is the least "clear" of any; the measure of evaluation is *some* function of the estimates of the probabilities of success and the values and the errors of these estimates.

Possible attacks on the pragmatic problem of induction are

1) Science ends in summarizing its evidence, and it has no part in the evaluation of policies. The objection to this viewpoint is that science obviously makes decisions of its own in both theoretical and applied science. Science must decide to take certain steps in its procedures, and these steps must presumably be evaluated by science. Since science does make decisions, the question is: How does it evaluate its own policies? Or is it unquestionably true that there are no undecided policy decisions of basic science? Are all the issues decided by what the "best" scientists actually do?

2) The evaluation of policies is a concern of science, but it is relative only to "given" values of the objectives and "given" attitudes toward risks. In other words, science does not estimate the values of objectives, since these must be given by "execu-

ve" or "popular" judgment. Hence, triple-uncertainty problems are meaningless to science. If this position is adopted, science itself can be evaluated nly on relative grounds.

3) Values can be assigned by science to objecives, and the criterions of best decision can also be ssigned. To accomplish this assignment and still ccept the circularity involved (science must accept alues to study values), it is necessary to develop a heory of science in which no scientific conclusion ver has complete validity and in which the methdology used by science is a self-correcting device. A typical model for this type of science has been eveloped within statistical quality control, and its pplication to scientific method is expressed in an rticle by Sebastian Littauer (*1*). "Ethical" coniderations relative to the values of the objectives lso have been discussed (*2, 3*). The general notion eveloped in (*1*) is that science does not come to onclusions, does not validate theories or verify hem, and so forth, but that science is essentially a control device. Specifically, scientific procedures provide policies of such a nature that if a decision is selected wrongly the procedure will indicate the incorrectness of the wrong solution earlier or more economically than any other method. That is to say, a method is scientific insofar as it presents controls at the best possible moment for the individuals involved. The perfect scientific method would thus provide perfect controls at every instant, and approximations to such an ideal are more or less scientific according to the degree of approximation.

This solution operates as follows. One assumes a criterion for the value of policies and uses it as a basis for the solution of the problem of induction. Evidence is then gathered and set into the total system—a system that includes a method whereby one can determine whether the evidence indicates the correctness of the "basic" assumption for evaluating policies. The circularity of the system then becomes a rather vast control mechanism by which the assumptions of the system are checked and their invalidity is determined at the earliest possible moment. A system would be "vicious" if it were such that no set of evidence statements could ever reveal the incorrectness of the assumptions by which the system operates. A system *could* be circular and nonvicious according to this definition. The pious hope or faith of those interested in science is that there exists a nonvicious circular system, that is, a system of scientific gathering of evidence, and evaluation of policies thereby, which continually provides for a self-adjustment of this system with a consequent closer approximation to the perfect controlled system.

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Remarks on Value Judgments in Scientific Validation

RICHARD RUDNER

Dr. Rudner, who is assistant professor of logic at Tufts College, Medford, Mass., served as discussant of the papers presented in this symposium.

AN important underlying point in the three preceding articles is the manner in which, if at all, value judgments impinge on the process of validating scientific hypotheses and theories.

I think that such validations do *essentially* involve the making of value judgments in a typically ethical issue. And I emphasize *essentially* to indicate my feeling that not only do scientists, as a matter of psychological fact, make value judgments in the course of such validations—since as human beings they are so constituted as to make this virtually unavoidable—but also that the making of such judgments is *logically* involved in the validation of scientific hypotheses; and consequently that a logical reconstruction of this process would entail the statement that a value judgment is a requisite step in the process.

My reasons for believing this may be set forth briefly, but before presenting them I should like to distinguish my thesis as clearly as I can from apparently similar ones that have traditionally been offered.

Traditionally, the involvement of value judgments (in some typically ethical sense) in science has ordinarily been argued on three grounds: (i) Our having a science at all, or, at any rate, our voluntary engagement in such activities, in itself presupposes a value judgment. (ii) To be able to select among alternative problems, or, at any rate, among alternative foci of his interests, the scientist must

make a value judgment. (iii) The scientist cannot escape his quite human self. He is a "mass of predilections," and these predilections must inevitably influence all his activities—not excepting his scientific ones. These traditional arguments have never seemed entirely adequate, and the responses that some empirically oriented philosophers and some scientists have made to them have been telling. These responses have generally had the following import.

If it is necessary to make a value decision to have a science before we can have one, then this decision is literally prescientific and has not, therefore, been shown to be any part of the *procedures* of science. Similarly, the decision that one problem is more worth while as a focus of attention than another is an extraproblematic decision and forms no part of the procedures involved in dealing with the problem *decided* upon. Since it is these procedures that constitute the method of science, the value judgment has not thus been shown to be involved in the scientific method as such.

With respect to the presence of our predilections in the laboratory, most empirically oriented philosophers and scientists agree that this is "unfortunately" the case; but, they hasten to add, if science is to progress toward objectivity, the influence of our personal feelings or biases on experimental results must be minimized. We must try not to let our personal idiosyncrasies affect our scientific work. The perfect scientist—the scientist *qua* scientist—does not allow this kind of value judgment to influence his work. However much he may find doing so unavoidable, *qua* father, *qua* lover, *qua* member of society, *qua* grouch, *when* he does so he is not behaving *qua* scientist. Consequently, a logical reconstruction of the scientific method would not need, on this account, to include a reference to the making of value judgments. From such considerations it would seem that the traditional arguments for the involvement of value judgments in science lack decisiveness.

But I think a different and somewhat stronger argument can be made. I assume that no analysis of what constitutes the method of science would be satisfactory unless it comprised some assertion to the effect that the scientist validates—that is, accepts or rejects—hypotheses. But if this is so, then clearly the scientist does make value judgments. Since no scientific hypothesis is ever completely verified, in accepting a hypothesis on the basis of evidence, the scientist must make the decision that the evidence is *sufficiently* strong or that the probability is *sufficiently* high to warrant the acceptance of the hypothesis. Obviously, our decision with regard to the evidence and how strong is "strong enough" is going to be a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis. Thus, to take a crude but easily manageable example, if the hypothesis under consideration stated that a toxic ingredient of a drug was not present in lethal quantity, then we would require a relatively high degree of confirmation or confidence before accepting the hypothesis—for the consequences of making a mistake here are exceedingly grave by our moral standards. In contrast, if our hypothesis stated that, on the basis of some sample, a certain lot of machine-stamped belt buckles was not defective, the degree of confidence we would require would be relatively lower. *How sure we must be before we accept a hypothesis depends on how serious a mistake would be.*

The examples I have chosen are from scientific inferences in industrial quality control. But the point is clearly quite general in application. It would be interesting and instructive, for example, to know how high a degree of probability the Manhattan Project scientists demanded for the hypothesis that no uncontrollable pervasive chain reaction would occur before they proceeded with the first atomic bomb detonation or even first activated the Chicago pile above a critical level. It would be equally interesting and instructive to know how they decided that the chosen probability value (if one was chosen) was high enough rather than one that was higher; on the other hand, it is conceivable that the problem, in this form, was not brought to consciousness at all.

In general, then, before we can accept any hypothesis, the value decision must be made in the light of the seriousness of a mistake, and the degree of probability must be *high enough* or the evidence must be *strong enough* to warrant its acceptance.

Some empiricists, confronted with the foregoing considerations, agree that *acceptance* or *rejection* of hypotheses essentially involves value judgments, but they are nonetheless loathe to accept the conclusion; instead they have denied the premise that it is the business of the scientist *qua* scientist to validate hypotheses or theories. They have argued that the scientist's task is *only to determine the strength of the evidence* for a hypothesis and not, as scientist, to accept or reject the hypothesis.

But a little reflection shows that the plausibility of this as an objection is merely apparent. The determination that the degree of confirmation is, say, *p* or that the strength of the evidence is such and such, which is on this view the indispensable task of the scientist *qua* scientist, is clearly nothing more

than the *acceptance, by the scientist, of the hypothesis that the degree of confidence is p or that the strength of the evidence is such and such*; and, as these men have conceded, acceptance of hypotheses does require value decisions.

If the major point I have tried to establish is correct, then we are confronted with a first-order crisis in science and methodology. The positive horror with which most scientists and philosophers of science view the intrusion of value considerations into science is wholly understandable. Memories of the conflict, now abated but to a certain extent still continuing, between science and, for example, the dominant religions over the intrusion of religious value considerations into the domain of scientific inquiry are strong in many reflective scientists. The traditional search for objectivity exemplifies science's pursuit of one of its most precious ideals. For the scientist to close his eyes to the fact that scientific method *intrinsically* requires the making of value decisions, and for him to push out of his consciousness the fact that he does make them, can in no way bring him closer to the ideal of objectivity. To refuse to pay attention to the value decisions that *must* be made, to make them intuitively, unconsciously, and haphazardly, is to leave an essential aspect of scientific method scientifically out of control.

What seems necessary (and no more than the sketchiest indications of the problem can be given here) is nothing less than a radical reworking of the ideal of scientific objectivity. The naive conception of the scientist as one who is cold-blooded, emotionless, impersonal, and passive, mirroring the world perfectly in the highly polished lenses of his steel-rimmed glasses is no longer, if it ever was, adequate.

What is proposed here is that objectivity for science lies at least in becoming precise about what value judgments are being made and might have been made in a given inquiry—and, stated in the most challenging form, what value decisions ought to be made.

The National Science Foundation

The National Science Foundation, an independent Federal agency, has been charged with a number of responsibilities related to science. The first one mentioned in its establishing Act of 1950 states: "to develop and encourage the pursuit of a national policy for the promotion of basic research and education in the sciences." The next section, dealing with the support of basic research, stipulates that the Foundation is to "appraise the impact of research upon industrial development and upon the general welfare."

To carry out these injunctions, the Foundation must have a broad conception of science that will transcend particular periods of time, or special areas of geography, or compartmentalized divisions of science. Above all, one must view science objectively, both in its historical perspective and in its social vision, as well as in its intrinsic philosophic relatedness. In this connection, I am reminded of a statement by Julian Huxley that "science is not the disembodied sort of activity that some people would make out, engaged in the abstract task of pursuing universal truth, but a social function, intimately linked up with human history and human destiny."

The role of science in human history can best be approached by studying both the history of science in its impact upon society and the history of society in its impact upon science. In connection with the renaming of History and Philosophy Section (L) of the AAAS in 1944, Henry Sigerist remarked: "Every situation in which we find ourselves is always the result of definite historical developments and trends of which we, as a rule, are unaware." Science itself has a past in which lurk shady and shadowy human factors. It is important that we examine the validation of scientific theories in terms of their philosophic and sociologic matrices.

The National Science Foundation is pleased that it could join the American Academy of Arts and Sciences and the Institute for the Unity of Science in cosponsoring the *Conference on the Validation of Scientific Theories.* It was particularly pleasing to have the first session form part of the program of AAAS Section L.

We all feel that the success of the conference can be measured largely by the stimulated growth afforded the participants by the interdisciplinary vistas that have been opened.—RAYMOND J. SEEGER.

International Cooperation to Improve World Agriculture

RALPH W. PHILLIPS

Dr. Phillips is deputy director of the Agriculture Division of the Food and Agriculture Organization of the United Nations, Viale delle Terme di Caracalla, Rome, Italy. He received his training at Berea College and the University of Missouri. In 1952, he was awarded an honorary D.Sc. degree by Berea College. Dr. Phillips served on the staffs of the University of Massachusetts and Utah State Agricultural College, where he was head of the Animal Husbandry Department, and the U.S. Bureau of Animal Industry, where he was in charge of genetics investigations. For 8 years he served as founding editor of the Journal of Animal Science. *During 1943 and early 1944, he served in the U.S. Department of State as an adviser on food production problems to the governments of China and India. Dr. Phillips has been a member of the FAO staff since December 1946, first as chief of the Animal Production Branch of the Agriculture Division and, since May 1949, in his present position.*

AGRICULTURE has undergone remarkable changes in the more highly developed countries during the last half-century. Developments in agricultural research have provided a sound basis for the application of agricultural science, or the group of sciences of which it is composed, in farm practice. Scientists from many disciplines have worked side by side in carrying forward this research. Extension workers have carried the new information thus obtained to farmers. The rapid progress that has been achieved would have been impossible without this teamwork.

A new type of teamwork has now emerged—teamwork among nations—in an effort to bring the benefits of agricultural science to an increasing number of the farmers of the world. Seventy-one nations are banded together as members of the Food and Agriculture Organization of the United Nations (FAO) and are working as a team to achieve this objective.

Cooperation between two countries, or among limited groups of countries, to improve agricultural production and rural well-being is not new. A substantial group of countries had banded together in the former International Institute of Agriculture (which was absorbed by FAO) to cooperate in the collection of statistics and exchange of other information. But international cooperation on the scale found in FAO, and including extensive intergovernmental consultation and action, as well as the exchange of information, is something new in the kingdoms of Ceres and Pan; and in a situation where populations threaten to outstrip food supplies (and, in some situations, water supplies), this universal effort to strengthen scientific research in agriculture and to bring the results of that research to farmers in every land is of concern not only to all scientists but also to all peoples.

This article is designed to set forth briefly the answers to three questions: What is FAO? How does the Organization carry out its work for the benefit of its member countries? What has it accomplished to improve world agriculture?

Organization of FAO

The FAO idea had its beginning at Hot Springs, Va., in May 1943. President Franklin D. Roosevelt had invited many countries to join in a discussion of world food and agricultural problems, and in this conference the idea of a permanent world organization to deal with these problems was crystallized and an interim commission was established. The Organization came into existence formally at the first session of the FAO conference in Quebec, in Oct. 1945, when the representatives of 32 countries signed the constitution.

The manner in which member countries have banded together and established an organization to assist in the solution of their agricultural problems may be understood more easily by an examination of the chart in Fig. 1. The member

countries, of which there are now 71, govern the Organization through the conference, which meets biennially. The conference, in turn, delegates certain functions to a council, composed of 24 member countries, during the intervals between conferences. The conference also selects the director-general of the Organization, who functions as its executive head. This post is now occupied by P. V. Cardon (Fig. 2) who was formerly research administrator in the U.S. Department of Agriculture. Under the director-general, the staff, which he appoints, is grouped in five technical divisions to plan and carry out the program of work which the conference approves. In subsequent sections of this article, reference is made only to the work of the Agriculture Division, although much of what is reported on methods of work and other general organizational matters applies also to other portions of the Organization.

The conference determines the annual budget of the Organization and the percentage of the total budget that each member country shall contribute. The annual budget has averaged approximately 5 million dollars since the Organization was established and was increased by the seventh session of the conference to approximately 6 million annually for 1954 and 1955. These funds are

FOOD AND AGRICULTURE ORGANIZATION OF THE UNITED NATIONS
composed of 71 countries which adhere to its constitution, participate in its program, and contribute directly to its supoprt

FAO CONFERENCE
composed of delegates from each of the 71 member countries; meets biennially to fix the budget, approve the program of work, and determine other policy matters

FAO COUNCIL
composed of 24 member countries elected by conference, delegates of which meet periodically between conferences to determine policy matters that cannot await conference action and to keep under review the state of the world's food and agriculture

DIRECTOR-GENERAL
elected by conference as executive head of FAO; is assisted by a deputy director-general

AGRICULTURE DIVISION | ECONOMICS DIVISION | FISHERIES DIVISION | FORESTRY DIVISION | NUTRITION DIVISION

PROGRAM OF WORK
including
the regular program and the expanded technical assistance program, the various subject-matter portions of both programs being carried out by the appropriate technical divisions, listed above, and serviced by the administrative services and the informational and educational services. These programs are carried out in close cooperation with, and for the direct benefit of, the member countries of FAO.

Fig. 1. The organizational structure, in broad outline, of the Food and Agriculture Organization of the United Nations.

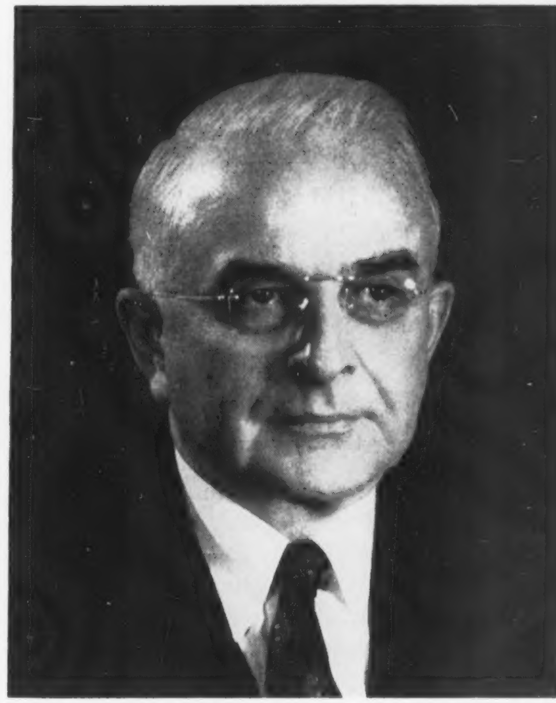

Fig. 2. P. V. Cardon, who is now serving as the third director-general of FAO. The first director-general was Sir John Orr (now Lord Boyd Orr), and the second was Norris E. Dodd, who had served earlier as U.S. Under-Secretary of Agriculture.

paid by governments direct to the Organization, usually in U.S. dollars or Italian lira.

The Organization also shares with most other members of the United Nations family of agencies (UN, WHO, UNESCO, ILO, WMO, ICAO, and ITU) in the operation of an expanded technical assistance program. This program is coordinated by a technical assistance board, in which each of the agencies participates with equal status. Funds for this program are appropriated by interested governments on a voluntary basis and are used by the various agencies for direct technical assistance to countries in accordance with a percentage division of funds agreed upon by the Economic and Social Council. The share allotted to FAO is currently 28 percent, and in 1953 this amounted to about $6,200,000. These funds, which come to the Organization indirectly, are payable in the currencies of the contributing countries. In many cases, these currencies have limitations on their convertibility, thus limiting the manner in which they may be used. In 1953, contributions were received in 34 currencies.

The headquarters phases of the FAO expanded technical assistance program are closely integrated with the regular program, while special temporary staff members are recruited to carry out field as-

signments in countries that have requested assistance and have signed agreements with FAO for the provision of assistance. Some direct advisory assistance to countries is provided under the regular program, but the funds available for this purpose in the regular budget are limited.

The headquarters of FAO is in Rome, Italy (Fig. 3), and regional offices are maintained in Bangkok, Thailand, for Asia and the Far East, in Cairo, Egypt, for the Near East, and in Washington, D.C., for the United States and Canada; subregional offices are maintained in Rio de Janeiro, Brazil, Mexico City, and Santiago, Chile, for Latin America. The Organization has three official languages—English, French, and Spanish.

Methods Used by FAO to Assist Its Member Countries

The methods used by any organization must, of course, be in harmony with the terms of reference of that organization and at the same time provide effective means of carrying forward its work. An international agency that exists to assist its member governments, such as FAO, must select methods that take into account the fact that the Organization has no sovereignty over these governments. It must be borne in mind, for example, that FAO owns no land on which it produces food, neither does it have any political control over, or direct advisory contacts with, farmers whereby it can influence the manner in which the farmers utilize their lands and market their products. Likewise, FAO does not own any laboratories in which it carries out research or in which it produces requisites for improving agricultural production, such as vaccines or insecticides. Neither does FAO own or control any agricultural schools or colleges in which it trains agricultural workers and leaders. All such activities, as well as others aimed at improving the efficiency and total output of agricultural production within countries, are the responsibilities of the governments of those countries. Hence, the methods selected by an international organization must assist the governments in carrying out their own functions most effectively, and the organization cannot in any way replace the governments in carrying out these functions.

The methods that FAO uses to assist its member governments in improving their agriculture are designed to meet the conditions described. They are:

1) Holding technical meetings, usually on a regional basis, but sometimes on a worldwide basis, in which delegates from countries exchange information and ideas and arrange for coordinated action on problems of common concern

2) Sending survey missions to study the needs of countries that may request such assistance and to propose programs for agricultural development

3) Sending individual experts or groups of experts to countries for periods of varying lengths to advise and assist the governments in planning and carrying forward technical projects

4) Providing limited amounts of technical sup-

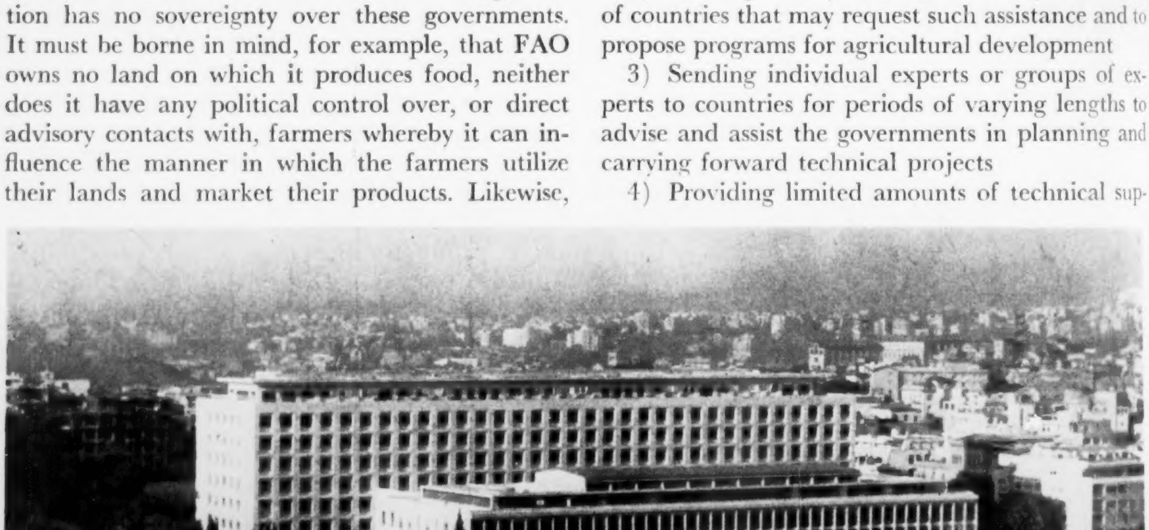

Fig. 3. FAO headquarters building in Rome. The low building in the foreground houses the conference facilities, the library, and one technical division. The remainder of the headquarters staff is housed in the building in the rear; and a cafeteria-restaurant occupies the top floor.

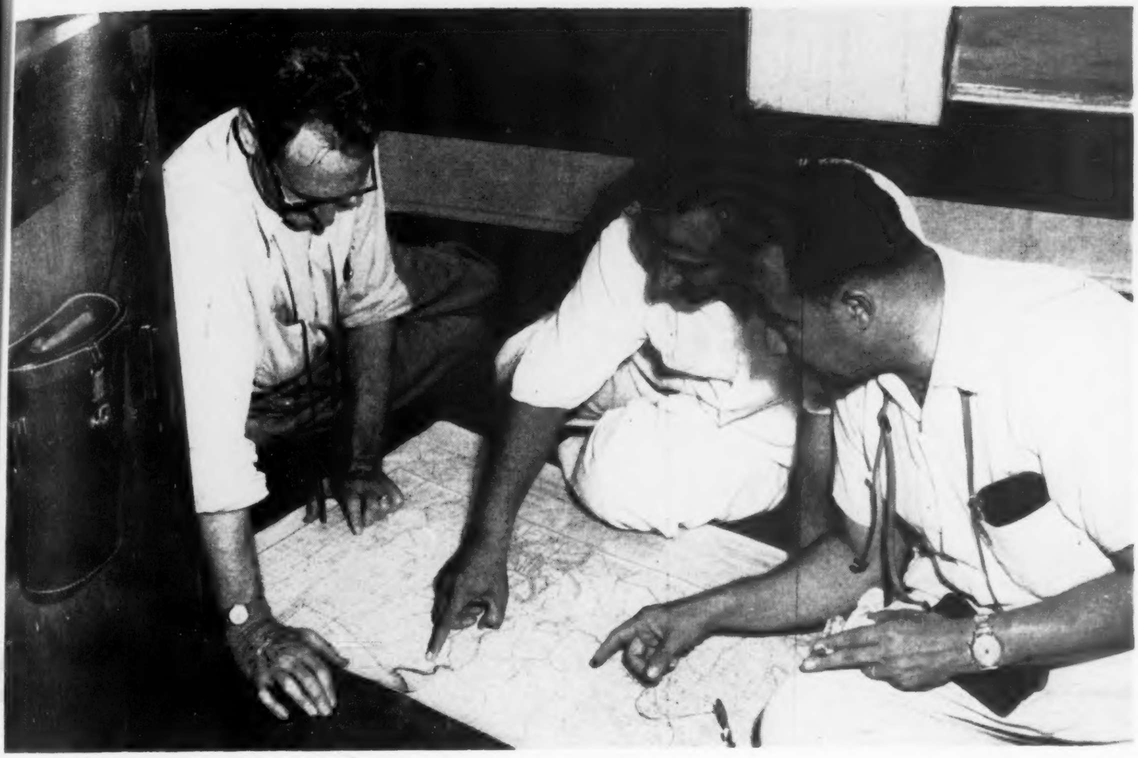

Fig. 4. FAO is advising the government of Pakistan on the development of an irrigation scheme in East Bengal. Here two FAO experts, from the Netherlands and the United States, go over the plans with a Pakistan official.

plies, equipment, and literature to enable experts serving in countries to carry forward their advisory activities in an effective manner, particularly by supplying specialized items that are not readily available in the countries concerned

5) Holding training centers for single countries or for groups of countries that have common interests in order to impart knowledge of specific techniques to trainees who will use them to carry forward specific agricultural development projects

6) Providing fellowships for study outside the home countries as a means of training the technicians and technical leaders who are needed to carry forward projects on which advisory assistance has been, or is being, given by experts

7) Preparing and publishing documents containing summaries of new technical findings and other materials for the guidance of member countries, particularly in planning and implementing projects for economic development

8) Maintaining contacts with the agricultural leaders in member countries through visits by staff members and correspondence to obtain information on their problems and to supply information and advice where requested

9) Organizing permanent technical bodies, as arms of FAO, to provide continuing mechanisms for consultation among countries that have common problems and for implementation of cooperative action programs to which these countries may agree

10) Assisting governments in preparing and formalizing conventions that may be required to lay the basis for common action as, for example, in the control of plant pests and diseases.

These methods provide a great deal of flexibility, yet conform to the Organization's terms of reference. In actual practice, the various methods are interrelated, and it is often necessary to use a number of these methods in dealing with any particular problem or set of problems, in order to achieve the most satisfactory results. Some examples of the manner in which each method has been applied in the agricultural program of FAO and an indication of some of the interrelationships are presented in the following section.

Progress in Assisting Member Countries to Improve Their Agriculture

The small staff that made up the Agriculture Division of FAO in its beginning was brought together in Dec. 1946. Much has been done in the 7 yr and some months that have elapsed. It is possible to mention here only a few of the accom-

Fig. 5. An FAO expert from the United States has been advising Saudi Arabia on the improvement of processing of dates; as a result dates are now being prepared for sale in modern packages, primarily to pilgrims to that country. The first packaged dates actually exported from Saudi Arabia are shown here being presented by the author (right) to the former director-general of FAO, Norris E. Dodd, who served in that capacity from mid-1948 to the beginning of 1954.

plishments in the field of agriculture as examples of the kind of work undertaken and the methods used. These examples are given in the same sequence as the listing of methods in the previous section.

International and regional technical meetings have been held to deal with many subjects in the agricultural field. One of these dealt with the production and field use of vaccines for the control of rinderpest, the most serious plague of cattle in the world. This meeting, held in Nairobi, Kenya, in 1948, brought information on newly developed techniques arising out of wartime and immediately postwar efforts to many countries, and provided the stimulus for further research and for the development of vaccine production and field control programs in several countries in Africa, the Near East, and Asia and the Far East.

Another technical meeting, held in Bergamo, Italy, in 1947, provided information to many European countries on the possibilities of using hybrid corn and resulted in a coordinated effort for the testing of United States and Canadian hybrids for adaptability to various European conditions. This meeting has been followed by six more at approximately annual intervals, the last being held in Belgrade, Yugoslavia, in Feb. 1954, to review the results of each year's work in the various countries and to plan tests for the next year. These meetings provided not only a great deal of stimulus for the use of hyrid corn in Europe and the Mediterranean area but also resulted in a coordinated testing program that provided a sound basis for a development program. As a result of follow-up action by governments, hybrid corn is already making a substantial contribution to increased food production in these regions. For the year 1952, it was estimated that the increase in yields from the use of hydrid corn amounted to approximately 273,000 tons valued at 24 million dollars.

In addition to the technical meetings, the project on hybrid corn has also included other types of activities, such as visits to countries by specialists, supplying experimental seed, and issuing publications to summarize the results obtained. These will be referred to later, but it should be pointed out here that this project is a good example of the type of thing that an international organization must select for inclusion in its program of work

if it is to be effective in assisting member countries. The Organization's activity has been essentially that of a catalyst, and the real work has been carried out in the member countries. This catalytic function on the hybrid corn project during the period from 1947 to the end of 1952 has cost the Organization a total of approximately $40,000. The increase in production resulting from the introduction of hybrid corn in the 1952 season was approximately 600 times this cost. Even so, hybrid corn had been introduced on only about 4 percent of the corn-growing area of Europe in 1952. Thus, the project is a type that gives large returns as a result of action by the countries themselves, yet it is low in cost to the Organization. The project is now being reorganized to place the major emphasis on the development of inbred lines for producing hybrid seed in Europe and, thus, avoid the cost of importing seed each year.

Survey and planning missions have been sent to Greece, Poland (before Poland withdrew from FAO), Thailand, and Nicaragua to assist these countries in planning their over-all agricultural development. In addition, more limited specialized missions have been sent to a few countries to carry out surveys, and FAO has cooperated with the International Bank for Reconstruction and Development in several planning missions, including missions to Uruguay, Ceylon, and Iraq, and with

Fig. 6. Many of the world's farmers still use small hand tools and implements to carry out most of their agricultural operations. Here an FAO expert from Switzerland instructs an Afghan farmer in the sharpening of a scythe.

Fig. 7. An FAO expert from the United Kingdom observes Ethiopian workers in the preparation of hides in connection with his advisory work aimed at improving the flaying, curing, and tanning of hides and skins in Ethiopia.

several other international agencies in a mission to Somaliland. This is not a complete listing, but it indicates the general scope of this phase of FAO's activities.

Some experts have been sent to member countries under the regular program, mostly for short periods, to advise on technical problems. In addition, a total of 68 experts served for the equivalent of about 55 man-years in nine member countries that were eligible for assistance under a special fund supplied to FAO by the United Nations Relief and Rehabilitation Administration (UNRRA) at the time that this temporary agency was completing its activities. However, most of the experts sent to member countries on advisory assignments have been supplied under the expanded technical assistance program, and at the end of Dec. 1953 a total of 279 agricultural experts had completed assignments in countries under this program, which was initiated late in 1950. Some of them served only short-term assignments ranging from a few weeks to 6 mo each, but the majority served for periods of 1 yr or longer. Also, at the end of Dec. 1953, approximately 150 agricultural experts were serving on assignments in 34 countries. These experts were recruited from many countries and have dealt with, or are dealing with, a wide variety of problems in animal production, plant production,

and land and water use fields. They are also working on projects aimed at improving government services for research and extension, facilities for agricultural cooperatives and credit, and improvement in processing agricultural products. A few examples of the advisory work of these experts are illustrated in Figs. 4–9.

In rendering advisory assistance, advantage has been taken of many opportunities to coordinate FAO activities with those of bilateral programs being operated in cooperation with the same countries by the United States under its Point IV (the former TCA), MSA, and other programs, which are now combined under the Foreign Operations Administration; by the British Commonwealth under the Colombo Plan; and by other countries. FAO activities have also been coordinated with those of other agencies, such as the Organization of American States. The objective of this coordination is to avoid overlapping and to insure that such activities supplement one another in order to promote the development of the countries receiving assistance.

FAO is not intended to be a supply agency and has no special funds for this purpose. However, under the expanded technical assistance program and to a very limited extent, under the special fund transferred from UNRRA, some equipment, supplies, and literature have been provided to governments in conjunction with advisory assistance and have included items not readily available in the country. The main purpose in supplying such equipment and supplies is to enable the experts serving in countries to get on with their work as rapidly and as effectively as possible. For example, in order to carry out a rinderpest-control program with modern vaccines, rather elaborate freeze-drying equipment is necessary, and this particular type of equipment has already been supplied to several countries where advisory assistance is being given on rinderpest control (Fig. 10). Another example of supplies provided to implement a project is the hybrid seed corn provided in connection with the European hybrid corn project mentioned in foregoing paragraphs. The seed supplied has generally been for use on experiment stations, but where countries required larger amounts for actual planting by farmers, it was either purchased directly by the government concerned or secured through a bilateral aid program, such as that carried out by the United States through the former ECA, and later MSA (now incorporated into the U.S. Foreign Operations Administration).

A number of training centers have been held in

Fig. 8. The work on control of rinderpest, like other activities of FAO, is designed to enable the nationals of countries to take over the activities as quickly as possible. Here an Afghan worker, who was trained by FAO experts, is shown vaccinating cattle in a village.

Fig. 9. An FAO expert from the Philippines served for a year in Thailand to assist the government in improving techniques of poultry production under tropical conditions existing in that country.

various parts of the world, and plans are under way for additional ones. Two of these training centers were held recently in India, one on rice breeding (Fig. 11) and one on the use of fertilizers to increase rice production; they were held for the benefit of all countries participating in the FAO International Rice Commission. Nine such training centers have been held by FAO under the expanded technical assistance program, and the Organization has participated in five centers organized by other agencies on agricultural subjects to provide training for workers from some of the less developed countries. In the nine FAO centers, training was given to 250 trainees. In addition to these short-term training centers, FAO has cooperated by providing experts to give instruction on agricultural subjects in two of the so-called fundamental education training centers that are operated by UNESCO on a relatively long-term basis for the benefit of the Latin American and Near East regions, and in three other such centers operated for the benefit of single countries, namely, Ceylon, Liberia, and Thailand.

No fellowships have been provided under the regular program owing to lack of funds for the development of an effective fellowship program, and only a limited number of fellowships was provided to a few European countries under the special fund transferred from UNRRA. The main effort at training promising workers has been made under the expanded technical assistance program, and, at the end of Dec. 1953, 110 agricultural fellowships had been completed, and 82 additional fellowships on agricultural subjects had been initiated. These fellowships are not for formal university study aimed at securing a degree but rather are aimed at providing specialized training and experience in some limited technical phase of the work upon which advisory assistance is being given to a country, so that the trainee may return to his own country and take an active part in the project concerned.

Two major series of agricultural publications are issued by FAO. One of these, called "Agricultural Studies," includes monographs that contain up-to-date summaries of information in fields that are important to a large number of FAO's member countries. They are designed for the use of the more highly trained technical leaders in these countries. Twenty-seven of these monographs had been issued at the end of 1953. Publications in a second series, called "Development Papers," are written for the policy-making officials in member countries, particularly the less developed countries. They con-

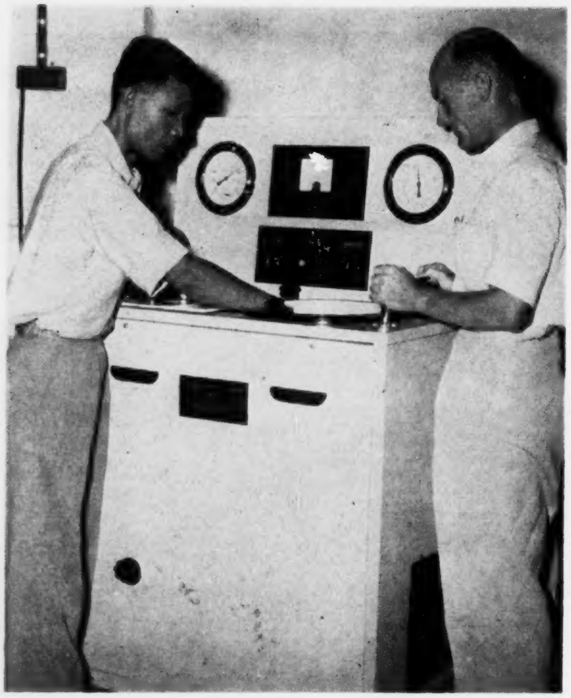

Fig. 10. In its program to improve the control of rinderpest, FAO has supplied modern freeze-drying equipment to several countries in order to enable them to produce modern vaccine. Here an FAO expert from Australia demonstrates to a Burmese veterinarian the proper method of operating the freeze-drying equipment.

tain information on subjects upon which action is necessary to increase agricultural production and in which FAO is attempting to assist its member countries. Forty-two of these papers had been issued at the end of 1953. Reports of some of the more important technical meetings are included in this "Development Paper" series as a means of bringing new information and/or plans for coordinated action before the agricultural leaders in member countries.

The FAO Plant Protection Bulletin is published at monthly intervals as a service to member countries under an International Plant Protection Convention. The purpose of this bulletin is to bring quickly to governments information on new outbreaks of plant pests and diseases, on steps governments are taking to improve their plant-protection services, and on new quarantine regulations affecting the movement of plant materials between countries. In addition, many informal working papers are prepared for use in technical meetings or for direct distribution to governments. Among these, special mention should be made of catalogs of genetic stocks of rice and wheat, which make available to breeders in all countries information on sources of breeding materials carrying specific characteristics that they may need in their experimental work.

Professional staff members of the Agriculture Division, employed under both the regular and expanded technical assistance programs and stationed at headquarters in Rome and at regional offices, now include 60 workers from 20 countries. (In the staff of the Organization as a whole, there are currently nationals of 50 countries.) These workers are in regular touch with many technical workers throughout the world with regard to problems in their several subject-matter fields, and by this means they give a great deal of useful information and advice to workers in FAO's member countries. They are also in regular touch with experts serving in member countries under the expanded technical assistance program, and through them provide countries with much information and advice. These staff members also prepare replies to the many official letters received from governments requesting information on various agricultural problems. They also participate in the handling of a large volume of correspondence that is required in the operation of other aspects of the Organization's program of work. This requires a substantial amount of staff time, owing not only to the wide variety of work under way, but also to the exacting protocol requirements of correspondence at the international level.

Fig. 11. Trainees at an FAO-sponsored training center on rice breeding listen to an explanation of how hybridization is being used in producing new strains. The training center was held at the Central Rice Research Institute at Cuttack, India, and trainees were present from nine countries.

Fig. 12. **A rice hybridization project is being carried out cooperatively by member countries of the FAO International Rice Commission, and the hybridization work is being done at the Rice Research Institute at Cuttack, India. Some of the experimental plants are shown here.**

Intercountry cooperation on agricultural problems is planned and implemented through FAO by organizing activities that fall essentially into four types and are of a more or less continuing nature. One of these has already been mentioned under technical meetings, where reference was made to a series of meetings on hybrid corn in the European and Mediterranean area. No formal organization has been developed, and the representatives of countries meet annually on the invitation of the director-general of FAO to review the work of the past year, and to plan the next year's activities. In other instances, more formal arrangements have been made, which are of three different types. These three types differ both in the extent to which a formal organization is developed as an arm of FAO and in the method of financing.

A Working Party on Mediterranean Pasture and Fodder Development is an example of a less formal organization. Each of the participating governments is invited by the director-general of FAO to designate an officer as a permanent member of the Working Party. This officer then represents the government in technical meetings of the Working Party and is the contact point for planning and exchange of information with the FAO staff and with members of the Working Party in other countries.

An example of a more formal type of organization is the FAO International Rice Commission. Work aimed at the improvement of rice production and utilization is carried out to a considerable degree under this commission, which is made up of 23 member countries of FAO to which rice is an important crop. The commission meets at intervals of 2 yr to discuss the over-all problems facing the countries and to decide upon programs of action. It has established a Working Party on Fertilizers and a Working Party on Rice Breeding in which experts from the member countries meet annually to consider technical problems and to recommend action to the commission. Costs of participation by governments in these meetings is borne by those governments, and FAO staff costs are borne by the Organization, as is the case with other technical meetings. In addition, the constitution of the commission provides for the carrying out of special projects, which all or a part of the governments may agree to finance on a cooperative basis. One

example of action being carried out, and to which a number of governments have made special contributions, is a rice hybridization program in an effort to combine desirable traits of the Japonica and Indica groups of rices (Fig. 12).

A further type of organization may be illustrated by the FAO European Foot and Mouth Disease Commission which has now been formed. It differs organizationally from the International Rice Commission primarily in that the operational costs are borne entirely by the participating countries through special contributions to a trust fund, which is administered by FAO.

FAO has sponsored only one international convention in the agricultural field. This is the International Plant Protection Convention to which reference was made in connection with the *Plant Protection Bulletin*. This convention was approved by the FAO Conference in its sixth session, 1951, and 19 countries have now adhered to it. In addition, FAO is custodian of five conventions, developed by the former International Institute of Agriculture, on locust control, marking of eggs in international commerce, standardization of methods of cheese analysis, standardization of methods of analyzing wines, and standardization of methods of keeping and utilizing herd books.

This brief description of the manner in which the Food and Agriculture Organization of the United Nations was formed and the way in which it is organized and carries out its work for the benefit of 71 member countries indicates how countries have joined together to deal with a problem of common, world-wide concern—the production of enough food for all and the improvement of rural well-being. The equally brief descriptions of work accomplished, which include only examples to indicate what is being done in the broad field of agriculture, indicate that it is possible to take effective international action in this field and that countries are cooperating in their attempts to deal with many common problems.

It is too early to evaluate fully the effectiveness of the efforts of FAO in the field of agriculture. The Organization has yet to reach its 10th birthday; and all the efforts in any program to improve agriculture cannot be expected to result in unqualified success. This is even more true in international than in national and state programs because of the additional political, social, and other differences that must be reconciled before a technical project can go forward effectively. Also, improvements in agriculture come about gradually, even in highly developed countries, and progress is inevitably much slower in countries that have few facilities for research and extension. Even so, there are clear indications that the member countries are using the Organization to an increasing, and reasonably effective, degree as a source of advice on technical problems and as a channel through which to arrange for intercountry cooperation on agricultural problems.

The Search for Unity

If we are to have a durable peace . . . if out of the wreckage of the present a new kind of cooperative life is to be built on a global scale, the part that science and advancing knowedge will play must not be overlooked. For although wars and economic rivalries may for longer or shorter periods isolate nations and split them up into separate units, the process is never complete because the intellectual life of the world, as far as science and learning are concerned, is definitely internationalized, and whether we wish it or not an indelible pattern of unity has been woven into the society of mankind. . . . In peace as in war we are all of us the beneficiaries of contributions to knowledge made by every nation in the world. . . . The best that every individual or group has produced anywhere in the world has always been available to serve the race of men, regardless of nation or color. . . . Whether it is mathematics or chemistry, whether it is bridges or automobiles or a new device for making cotton cloth or a cyclotron for studying atomic structure, ideas cannot be hedged in behind geographic barriers. . . . The fundamental unity of civilization is the unity of its intellectual life.—RAYMOND B. FOSDICK.

The Trouble with Science Courses

BRUCE STEWART

The author, who is an associate professor in the biology department of Missouri Valley College, Marshall, Missouri, has been experimenting with foundation courses in physical and biological science for the past 5 years. He has for many years been interested in the interrelationships of the natural and social sciences.

Conventional instruction in science has given scant consideration to the relation of the work of the scientist to the social order of which it is such an important feature. In fact, scientists, like their fellow men in other walks of life, have only recently become conscious of the fact that ours is a science-centered culture, a civilization in which a technology based on science has shaped our lives and our thinking, often in subtle and unobserved ways.—E. J. McGrath.

VERY few people, and practically no scientists, question the desirability of exposing students in secondary and higher education to the subject of science. Considering the way in which science has revolutionized the material world, it would be difficult to rationalize any serious arguments against the development of such an acquaintance.

There is, unfortunately, much less accord on how this is to be done or what aspects of the subject are most important. To help in deciding on these points, we must be clear about all the things we are trying to accomplish. After identifying these, we can come to some definite conclusions on what is needed.

Since most students will not become scientists and do not have the time, ability, and interest to specialize, it has long been educational practice to provide general-science courses to meet this need. These courses first appeared in the intermediate grades and worked their way up to the college level.

An attack was made against survey courses as superficial and routine memory work without laboratory training. Consequently, when the general education program came into being, it attempted, in many cases, to establish "foundations" or "basic" courses in physical and biological sciences with associated laboratory work.

Complaints were soon forthcoming, primarily from scientists, about this arrangement. The charge heard most often was that these courses did not instill any real appreciation of the methods and development of science but were merely dressed-up memory routines, with equally stale laboratory parrot-work in which cookbook recipes were rehashed and the spirit of discovery was massacred.

Some schools tried to adapt physics (or other) courses for this vital function, using the historical approach and correlating reading and laboratory work with the great discoveries made in the past. One such effort (*1*) called this the "block-and-gap" method, in which integrated "blocks" of discoveries from the various physical sciences could be investigated thoroughly and ingeniously. This program centered around physics and left most scientific knowledge (the "gaps") completely unexplored.

A Problem and Its Present Alternatives

In consideration of the mountains of knowledge in each of the many sciences and the limited time that the average curriculum and student have to spend on these subjects, it is an inescapable conclusion that a choice must be made: The training must either hit most of the high points briefly and simply or become intensive in selected cases, leaving the rest as vacuum.

On behalf of the block-and-gap method, it may be said that the methodology has a better chance of assuming real meaning and significance. It provides an opportunity to understand what really happened in the past and to recapture the times, even if such a tiny segment is used.

Against these advantages may be placed a serious shortcoming. The gaps are so profound that the student can hardly be considered to have a working knowledge (however elementary) of science as a whole; and, as far as content is concerned, he can be regarded as educationally lopsided.

Before condemning extensive treatment, it would be well to inquire whether a person should have

some idea of what he is seeing when he looks at the earth, the sky, and the many functions of the human body. Should chemical formulas be resigned to complete gibberish? Should all the physical phenomena of daily life not included in the intensive "blocks" be relegated to utter darkness?

Time is required, not just for the learning of material, but also for its integration. It is a fairly simple process when restricted to a limited number of blocks; however, the time necessary for integration mounts almost geometrically with the increase in concepts introduced. Any expectation of a thorough interrelating of concepts in the latter case must remain a false hope.

For 5 years, I have taught two foundations courses, one in physical science, the other in biological science, for college freshmen and sophomores. The outline prepared was restricted to the outstanding concepts (in my opinion) and those most likely to be encountered by the average person. Covering this ground in the allotted time was a feat in itself for classes almost totally ignorant on the subjects. A complaint was made by an educationalist that not enough time was given to integration.

With this it was easy to agree, but before one can undertake integration he must have something to integrate and sufficient apperceptive mass to make it real. A good deal of integration was achieved by careful construction of the sequence of material, so that there was a pyramiding effect and what had gone before was often essential to the understanding of the present. Some interrelationships were also brought out directly.

Thus we come to one of my main conclusions: There is nothing wrong with a science course whose avowed function is (in part) to provide a necessary background of usable fact unless it is assumed (i) that the training has to stop with this, (ii) that such courses necessarily teach "the scientific method," (iii) that integration is given a real chance, and (iv) that the associated laboratory work is truly "experimental."

In other words, there are many functions that a truly adequate science training should serve, a minimum working factual background being only one of them, but one not to be underrated or overrated. There is, of course, no reason why historical treatment, techniques, or other aspects of science training cannot be combined with information.

An interesting piece of work is reported by Kruglak (*2*), who begins by observing that the thought processes used by scientists in solving problems have been sadly mangled and stereotyped by the writers of science textbooks and workbooks. He gives illustrations to show how teaching methods are antithetical to the spirit of the subject they are trying to teach. Finally, he alludes to the use of laboratory work that requires the student to think his way through novel and thought-demanding problems involving the subject matter.

Other endeavors with different combinations and emphases could be considered, but all of them would leave us with the same questions. The first is: What things should training in science do for the nonscientist? After deciding this and taking into account the time available, then: Which of these aims is most important? Or, more probably, what combination of them is best?

A Classification of Objectives

It would appear that we can classify all objectives of nontechnical science training into five categories. Each will be discussed briefly but not necessarily in the order of their importance. Also, it must be emphasized that such separate consideration does not mean that they should be taught in isolation.

1) *Appreciating the development of science.* The development of an appreciation of how science came to be is an important objective. The treatment is cultural and historical in nature, being an effort to recapture the spirit and circumstances of the epochal discoveries of the past. With the block-and-gap method, a selected series of related concepts are followed out. Otherwise, there is a running account of the whole story.

When laboratory work is used, there is generally an effort to duplicate the historical sequence and tie it in with the classroom account. Techniques and attitudes may readily be drawn into this theme, even though it may be objected that repeating an experiment about which the students have just read is not entirely new.

2) *Building up information.* Since an immense amount of time would be required to duplicate science history operationally, the majority of textbooks, in an effort to cover the whole field, treat science as a routine learning process. A variety of organizations are used, but main concepts are explained, illustrated, and (to a greater or lesser extent) integrated. If the historical theme is involved, it is used only briefly in the introduction to the subject headings

The laboratory work is too often standardized to the traditional experiments and either the student knows what he is going to find before he finds it, or if he does not know anything about the subject he just tries to follow the recipe. In this case, he is only dimly aware of what it is all about. A typical student query would be: "Is this what I am supposed to get?"

This is to be expected if it is realized that when the particular discovery in question was originally worked out, it took much time and thought on the part of a first-class mind to do what we now require of a second- or third-class mind in a couple of hours. This is a serious barrier to actually duplicating the historical process.

Laboratory guides need not be organized as routine directions on what to do. They can be composed of a sheet of questions that begin with what is known and force the student to reason out the problem involved, perhaps a hypothesis or two, and even what he would need to do in order to gain data necessary for forming or testing a hypothesis on the subject. I have found this method successful.

3) *Developing techniques and attitudes.* Nearly every textbook and laboratory guide has a section on the scientific method. Here the techniques, attitudes, and assumptions are codified, packaged, and readied for quick and easy consumption. The textbook almost invariably puts the scientific method at the very first, when there is no apperceptive mass, and consequently it degenerates into a memory exercise.

Perhaps a brief history may be given of how the modern outlook evolved, but generalizations of this kind can assume signficance only after repeated contact with experiences that are specific and meaningful.

The point of contact for the most specific experience is the laboratory. In order to understand scientific procedures, the student must endure the pain of thinking with ingenuity in order to get through a problem situation (one appropriate to his ability). This demands even more ingenuity on the part of the instructor or author and here is the reason there is so little of it being done, as Kruglak has emphasized. Perhaps in this way we would also learn that the scientific method is not an easy formula for transforming morons into geniuses.

4) *Understanding the effects of science.* Here we will be accused of getting somewhat afield, and perhaps so from the view of orthodox physical and biological science. However, our stated objectives require that we consider the neglected subject of understanding the effects of science. If the average man must choose which will touch his life more intimately, a knowledge of nuclear chemistry necessary to understand how an atomic bomb goes off, or the atomic effects on his life and society, he cannot avoid choosing the latter. The matter does not end here, for as competent scientists have observed, "What of importance can be named about our world today that has not been created or revolutionized by the effects of science?" There is nothing, and unfortunate as it may seem, quite a number of these effects are touched with great difficulty and danger.

Since many natural scientists are uninformed on this topic, they regard it suspiciously as a frill, a fad, or a way of making science a "snap." One scientist called it "the romance of production and the glories of the kilowatt." Far from being an area for wool-gathering and happy speculation, this is a place where many concrete and unpleasant facts must be faced and where there is a desperate need for straight and fearless thinking.

It may be objected that this is the business of sociology. In theory that would be the place for it, but it is not being done there, perhaps because sociologists as a whole are concerned with more specialized subject matter. In my physical-science course, I include a few of the more elementary effects, and my students do not regard this part as easy. This topic is readily adapted to either intensive or extensive methodology.

5) *Applying the scientific method in social problems.* Here assuredly we will be charged with being clear off the track, but again we are speaking about aspects of science most vital to the average man without regard for where they may be studied. At this point, we come face to face with the profound gap in our culture—the fact that the physical and biological sciences have had a head start of several hundred years on social control and utilization.

In addition, the older sciences have been subsidized and encouraged to criticize, reconstruct, and come up with disturbingly different ideas. The failure of the attempt by physical scientists to ignore such developments is seen in the flurry of social awareness generated among scientists by atomic energy during the last decade. That much remains to be done is evident in the exclusion of this subject from the National Science Foundation, and we may be sure that the problem will grow in dimensions rather than diminish.

Included in my biological-science course is 2 weeks' work in which certain biological and biosocial knowledge, as well as scientific methodology, is applied to selected problem situations. Fundamental guideposts and conclusions are established as illustrations of what can be done.

A condensed outline of some of the material used in connection with objectives 4 and 5 follows.

Social Effects of Science

1. The industrial revolution as a result of applied science, its spread across the world
2. Effects of this revolution: dehumanization; automa-

tism; major social problems traced back to this origin
3. Antisocial effects of basic discoveries: dynamite; indigo dyes; airplane; atomic energy, etc.
4. Cultural lag as a cause
5. Responsibility of scientists to society: examples, including the program of the atomic scientists
6. The New World made necessary by physical science: effects of science that compel integration and planning, world government, and intercultural harmony
7. Invention and the patent system: its importance, changing character, and obsolescence
8. The forces of culture resulting from science—their nature and effects

Social Biology

1. The nature and function of law in natural phenomena: universal processes; basis of successful prediction and control
2. Physiological drives as universal processes (laws): their nature and manifestations; their cultural forms
3. Activity drives and others: their nature and manifestations; their cultural forms
4. Interrelationships of the foregoing
5. Penalty for pursuing policy contrary to laws: social failures as a result
6. Application to specific problems: kind of government made necessary; the fate of war. (Others as time permits)
7. Ecological influences—population pressures

Some Proposals and Conclusions

The five headings used here constitute one way of classifying the objectives of science training for nonscientists. There are undoubtedly other ways of organizing the subject matter. But regardless of how it is classified, several conclusions will stand out with unmistakable clarity.

1) Painfully obvious is the fact that any effective treatment of these objectives would require far more time than is allotted to the task. Science courses are expected to accomplish too much in too little time. Science teachers are being loaded with the Labors of Hercules but with hardly the strength or any real opportunity to perform them.

If more than one, perhaps two, of these objectives are to be achieved, then a curriculum change is in order. If others wish to interpret this as mere partisanship, let them compare the accomplishments, the effects on daily life, and the method of their subject with those of science.

It may well be that both intensive and extensive methods should be used, depending upon which of the objectives a specific course is attempting to realize. Until adequate time is available, a selection will have to be made.

2) The social effects and applications of science deserve far more attention than they have received. They must be taught somewhere; in the general-science work if need be. The oft-heard objection of orthodox scientists is that if students are trained to be scientific in chemistry they will automatically be scientific everywhere else. In order to answer this charge, it is necessary to consider the following conclusions justified by transfer of training experiments: (i) The old idea of Latin or science as an effective means of improving the general function and discipline of the mind is not supportable. (ii) The amount of transfer occurring correlates well with intelligence. (iii) It has little correlation with fund of factual knowledge, although one investigator did find a high correlation between facts and the ability to distinguish valid explanations of common phenomena. (iv) Transfer is improved if generalizations are identified. (v) It is improved still more if the student is given specific direction. In general, it may be said that for most people very little transfer occurs without considerable help and direction. This is one reason why I attempted in my courses to provide this help in objectives 4 and 5. Apparently these must be taught directly if we expect them to be learned and used.

When the Watson-Glaser Tests of Critical Thinking were administered before and after similar exposures, there was found a measured increase in scores. As might be expected, learning such methods and attitudes proceeds best when dealing with specific cases.

3) An over-all plan for science training has become essential. Little has been accomplished toward an integrated plan, partly because of intrascience departmentalized thinking. Coordination with the social studies is especially urgent in view of their mutual relationships with the overpowering and perilous gap in our culture.

The opposition to reconstructing without regard to the walls separating departments is formidable. Yet it is here that much productive advance is to be made. Anyone who in the past attempted such a project was, as one penetrating observer remarks (*3*), "thwarted by all full professors and heads of departments concerned because his newfangled ideas would not fit into the sacred scheme of academic curricula and budgets. But the departmentalization of modern science does little to foster, and much to discourage, students to go for adventures in new lines."

There is need for a criterion or measuring standard for determining the most effective synthesis of objectives and methods. If one has been proposed here, it is probably that which aids people most

effectively to understand and live in the modern world.

4) The general education science program has been treated as the scientific orphan, or perhaps as an offspring whose legitimacy is not quite certain. This is especially odd when it is remembered that general science must be the point of contact with the subject for most people.

There are trumpet calls for something new and different, but where are the reading materials for these innovations, especially with classes of 50 to 100 or (shudder) even more? For every textbook salesman with something to offer along this line, there is 0.01 of a textbook salesman with appropriate laboratory aids.

The dilemma in which we find ourselves and the one to which general education science training should address itself has been well stated by Lancelot Hogben (*4*): "The training of the statesman or the man of letters gives him no prevision of the technical forces which are shaping the society in which he lives. The education of the scientist leaves him indifferent to the social consequences of his own activities."

This also applies to the ordinary citizen. "Classical" scientists may disagree, but unless real headway is made toward closing up the ominous social lag, we may not be around long enough to argue about it. A very apt conclusion is phrased by former U.S. Commissioner of Education, Earl J. McGrath (*5*), which is a continuation of the introductory quotation:

> It is increasingly apparent that unless the proper social controls can be exercised the latest work of the scientists may nullify all the benefits to human kind that have flowed from the activities of their fellow workers in earlier days. Hence courses for those who will not be scientists or physicians or engineers, but who nevertheless in a democracy determine social policy which may wreck modern culture or destroy the scientific enterprise itself, must, a growing number of scientists are agreed, instruct students in the social implications and consequences of scientific work.

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American Academy of Arts and Sciences

It was an honor for the American Academy of Arts and Sciences to participate as one of the sponsors of the *Conference on the Validation of Scientific Theories,* the initial papers of which appear in this issue. President Land, of the American Academy, unfortunately was away from Boston at the time of the opening meeting, and so it became my privilege to convey the Academy's greetings to the conference.

In this I found some minor appropriateness for, as it happens, I am the vice president for the Humanities in the American Academy. This was a conference that pertained largely to matters of science. There was a time in history when the humanists would have seen to it that no men of science were present at such a discussion. There was a more recent time when perhaps the men of science would not have welcomed humanists. Now we have reached a time when it is quite apparent to everyone with any sense that the cause of scholarship needs cooperative, and not competitive or even antagonistic, action.

Yet these bridges are very difficult to build, and many a good effort has failed. We must keep on trying. For it seems beyond the necessity of proof that science has a very serious effect upon human conduct and also that human conduct may have a profound effect upon science. It was to explore such questions that scientists, social scientists, and humanists met together, bearing the virtues of their respective disciplines without brandishing their parochial standards.

We do not know whether Count Rumford would have approved of these meetings or have sat through them in his day; but we know that the present membership of the American Academy of Arts and Sciences did approve of them and looks for much good to result.—JOHN E. BURCHARD.

Signals through Space

WILLIAM L. ROBERTS

Mr. Roberts, who is a native of England, has been an engineer in the Westinghouse Research Laboratories, East Pittsburgh, Pennsylvania, since 1948. After graduating from Emmanuel College, Cambridge University, in 1941, he worked on radar for the British Ministry of Aircraft Production until 1946 and then was associated with the International Telephone and Telegraph organization in London and Nutley, New Jersey, until he joined Westinghouse.

SINCE the days when Marconi, Poulson, Fessenden, and others brought long-distance radio communication into reality, the possibility of interplanetary and interstellar radio contacts has been a fascinating topic of speculation. At long last, it was felt that if the other planets were inhabited by people possessing a technology equal or superior to our own, then we could establish telegraphic or telephonic intercourse with them. Such dreams have not yet materialized, not because we do not possess the necessary transmitting and receiving equipment, but because our neighboring planets either are not inhabited or their occupants cannot or do not want to communicate with us.

Nevertheless, radio signals are constantly reaching the earth from the celestial sphere around us, and even though these signals are not the kind that science fictionists would particularly appreciate, they are of great academic interest and may someday be of great practical importance. For radio astronomy, as this recently developed study of extraterrestrial radio signals is now called, is complementary to visual astronomy and provides observers with a new tool for the study of the heavens.

Figure 1 illustrates the electromagnetic spectrum extending from the long radio waves with wavelengths of hundreds of meters to gamma rays produced by radioactive materials that have wavelengths less than one hundred-millionth of a centimeter. Normally, the human eye can view satisfactorily radiations in the visible portion of the spectrum, which extends from a wavelength of about 4×10^{-5} cm (an extreme violet color) to 7.2×10^{-5} cm (a deep red color). Such is the very small portion of the spectrum to which the human eye is responsive, but by the use of instruments, such as thermopiles, bolometers, and photoelectric cells, the infrared and ultraviolet portions of the spectrum may be studied. However, for astronomical observations, the earth's atmosphere absorbs much of the ultraviolet and infrared radiation, so that the effective "window" of the electromagnetic spectrum centered on the visible portion extends only from about 10^{-3} cm in the infrared to 10^{-5} cm in the ultraviolet. Yet, the so-called "radio window" of the spectrum extends from about 0.25 cm to 20 m, with the atmosphere absorbing wavelengths less than 0.25 cm and the ionosphere reflecting those longer than 20 m. Thus, if the width of such a window is expressed as the ratio of its upper to its lower wavelength, then it may be seen that the radio window is considerably wider than the optical window. Because of this, the former is now beginning to assume great importance in astronomy.

Since the last century, it has been known that all bodies at temperatures above absolute zero (0°K or −273°C) radiate energy over a wide range of frequencies in the electromagnetic spectrum (including the radio portion of it) and that the hotter the body, the greater the intensity of the radiation. When the radiating body is a perfect emitter and absorber of radiation, it is known as a "black body"; and although such a perfect emitter and absorber is not found in practice, it may be closely simulated in the laboratory by a small hole in the shell of a hollow sphere with a blackened interior.

The intensity of the radiation emitted by a black

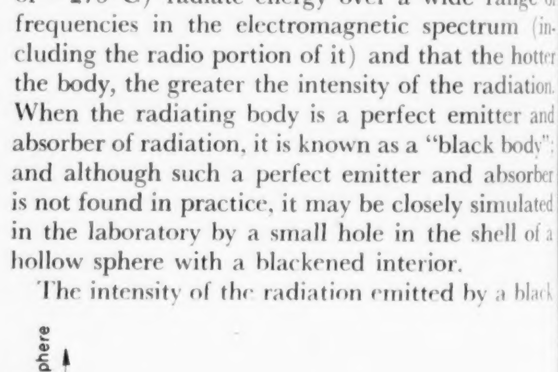

Fig. 1. This diagram illustrates the transparency of the earth's atmosphere for the various types of radiation in the electromagnetic spectrum.

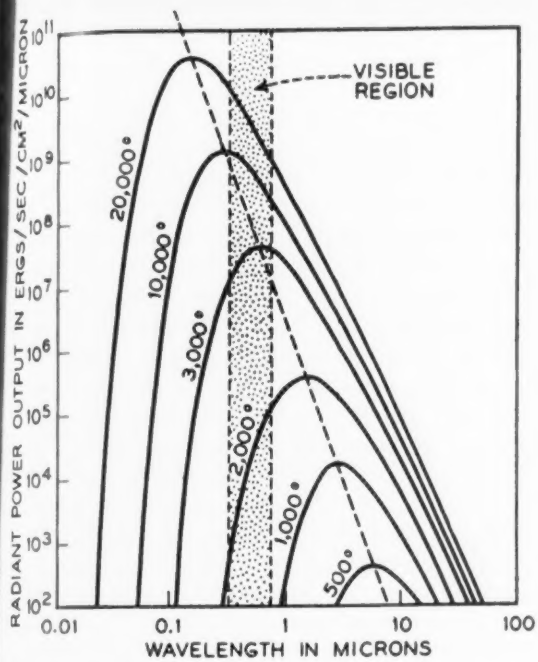

Fig. 2. Black-body radiation curves for temperatures between 500° and 20,000° K. (1μ = 10,000 A = 1/10,000 cm)

body at different temperatures as a function of wavelength is shown in Fig. 2. From this illustration it is seen that the radiation for a black body with a temperature above 2800°K (2527°C) and below 9000°K (8723°C) reaches a peak intensity in the visible part of the electromagnetic spectrum and that the higher the temperature, the shorter the wavelength at which the maximum occurs. It is therefore possible to determine the temperature of a black body by merely measuring the wavelength or frequency corresponding to peak emission. Thus, it is not necessary to make physical contact between a hot body and a suitable thermometer to establish the temperature, because as long as its radiations can be detected and accurately measured at a distance, its temperature can be calculated.

When it was realized that the temperatures of bodies could be so measured, the sun naturally became a subject of study. When viewed through a suitable telescope, the sun appears as a bright disk with a sharp periphery. This is the so-called "photosphere" with a diameter of about 864,000 mi. Outside the photosphere is an atmosphere of luminous but almost transparent gases. The lower part of this atmosphere, extending to a height of a few hundred miles above the photosphere, is called the reversing layer, and above this, extending to several thousand miles, is the chromosphere, consisting mainly of the lighter gases. Outside the chromosphere there is another envelope of very great height and very small density. This is the corona, which is best studied during a total eclipse. The various regions of the sun's atmosphere and the corona are illustrated in Figs. 3 and 4, respectively.

In 1903, Langley made measurements on the emission from the sun up to wavelengths of 0.0003 cm and found its apparent temperature to be about 6000°K (5727°C). Adel, in 1942, confirmed these results by measurements of radiations with wavelengths up to 0.0024 cm. Naturally, these wavelengths were much smaller than radio wavelengths (which for practical purposes are considered greater than 1 cm). However, from the experimental results obtained by Langley and Adel, it is possible to calculate whether or not the energy emitted from the sun in the radio portion of the electromagnetic spectrum could be detected here on earth. At a wavelength of 1 m and using a receiver with a band width of 2 Mcy/sec, the power received, per square meter, at the earth's surface is approximately 10^{-17} w, if we assume the sun to be a black body with a temperature of 6000°K. Unfortunately, this radiational level is approximately 1000 times too small to be detected by a good receiver with a normal antenna. However, it was believed that, by reducing the wavelength to about 10 cm and by using a high-gain antenna system, such radiations could be detected.

After 1940, several investigators strove to detect the noise radiation from the sun. In 1944, Reber

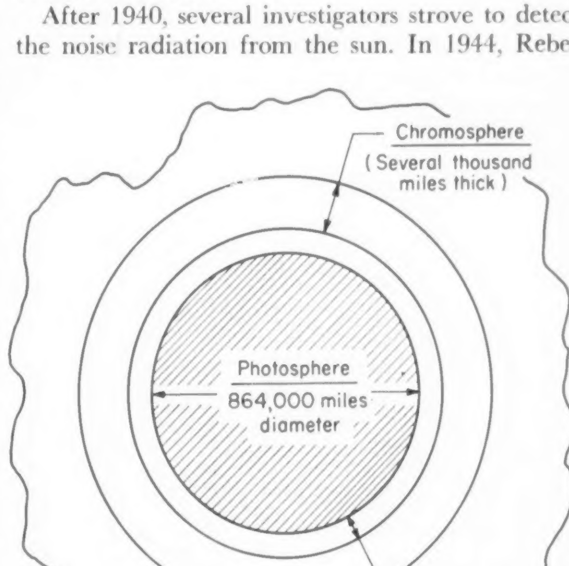

Fig. 3. Sketch of the sun and its atmosphere (not drawn to scale).

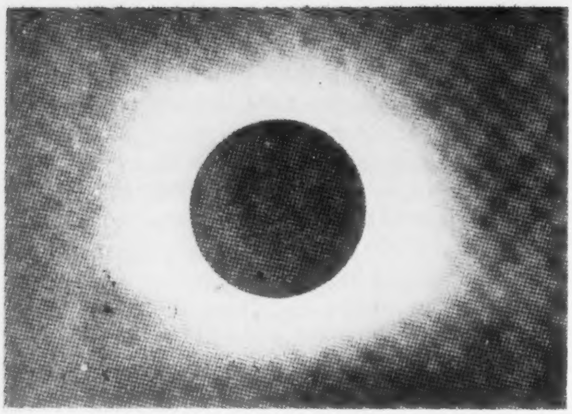

Fig. 4. Solar corona at the eclipse of 28 May 1900.

finally succeeded in measuring this radio noise; he believed that his results confirmed the black-body theory, with the apparent temperature of the sun being about 6000°C. Southworth, in 1942 and 1943, using shorter wavelengths, established the apparent temperature of the sun as 18,000°K, and his work was soon confirmed by other scientists in the field. As a consequence, it is now believed that the corona of the sun, with its higher temperatures, is the primary source of the radio noise emission.

In 1936, various radio amateurs reported "hissing" sounds in their receivers in the 10 to 40 Mcy/sec range. This noise was received only in the daytime and was often followed by radio fadeouts and bright eruptions on the surface of the sun. From the black-body theory, it can be seen that the normal radiated noise level at a wavelength of 1 m is about 1000 times too small to be detected. Yet this noise detected at much lower frequencies by the amateurs, if it came from the sun, would have to be about 10,000 times greater than calculated on the black-body theory, assuming that the apparent temperature of the sun is about 6000°K.

Soon the reports of the radio amateurs were confirmed by Arakawa in Japan, by Dellinger in the United States, and by Newton and Barton in Great Britain, all of whom demonstrated the correlation of high noise and radio fadeouts. Moreover, in Feb. 1942, the operation of British gun-laying radar, working on wavelengths of 4 to 6 m, was seriously disrupted by interference apparently caused by a large sunspot crossing the solar disk. Although it had been assumed that the sunspots were responsible for the observed terrestrial effects, it remained for Ryle and Vonberg, using specially designed antennas, to show that the abnormal radiation did actually come from a portion of the surface of the sun with an area about equal to and coincident with that of the sunspot. From their data, Ryle and Vonberg estimated that if the sunspot were a black-body radiator, it would have to have a temperature of 10^9°K to account for the observed results. Such surface temperature on the sun is inconceivable, because the temperature in the depths of the sun is believed to be about 1000 times less than this value. Thus, in the case of the abnormal emissions from sunspots, the black-body radiation theory is not adequate to account for the observed effects, and some other mechanism must be postulated.

Two rather elaborate theories have been devel-

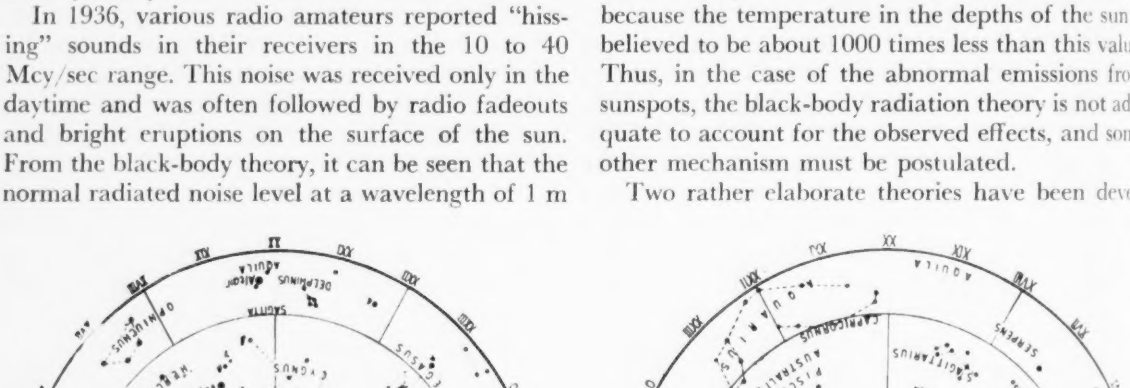

Fig. 5. Polar projections of the heavens showing the constellations of the Northern Hemisphere (left) and the Southern Hemisphere (right).

oped to account for the abnormal radiation. One, developed by Ryle, assumes an electrostatic field at the surface of the sun. This electrostatic field gives the free electrons present in the sun's atmosphere increased energy, and in times of sunspot activity, the electrostatic field and the electron energies are increased in certain areas of the sun's surface to a sufficient extent to account for the abnormal radiation. The other theory assumes special types of electron oscillations in the sun's atmosphere and has been proposed by Martyn as a development of electron gas studies originally made by J. J. Thomson. The difficulty involved in this theory lies in the fact that no mechanism has been proposed whereby these oscillations could be maintained.

While there is general agreement that the black-body radiation theory can account for the radio noise emitted from the sun during quiesence, there is no such agreement on the cause of intense emissions during sunspot activity. Yet the science of radio astronomy is still very young, and undoubtedly future theoretical and practical work will solve these problems.

Until about 300 yr ago, it was generally supposed that the earth was at the center of a great hollow globe, or celestial sphere, that the stars were affixed to the inner surface of it, and that it made 1 rev about its axis every 24 hr. The stars were considered affixed to its inner surface, not in an orderly or even in a perfectly random manner, but in distinctive groups, or constellations, to which names of animals and figures of ancient mythology were applied. To an observer in the Northern Hemisphere, the stars or the celestial sphere appear to rotate about a point very near to Polaris, or the Pole Star, and hence polar-projection star maps, such as Fig. 5, were drawn up centuries ago. The invention of the telescope soon made it evident that some stars were at much greater distances from the earth than others and that the celestial sphere did not in fact exist. However, as a means of charting the positions of stars, some of these old concepts are very useful and have been retained in modern astronomy.

The sun and its planetary system are nowadays considered part of a tremendous assemblage of stars known as a galaxy. From the groupings of the stars, we are led to believe that the galaxy is disk-shaped, as illustrated by Fig. 6, with a diameter of about 100,000 lt-yr (light-years) and some 20,000 lt-yr thick at the center. The sun, a very average star, is located about 30,000 lt-yr from the center of the galaxy; when an observer looks toward the Milky Way, he looks along the plane of the disk.

Our galaxy is only a localized system or part of the universe, and other galaxies are known to exist. Another great star system is the Andromeda Nebula, the nearest of the spiral nebulae, and about 750,000 lt-yr distant from us. In size and shape, it is believed to be very similar to our own galaxy. Large telescopes have shown these external galaxies to be fairly well scattered in space and within a range of 500 million light-years from us; there may be as many as 100 million of them.

The discovery of galactic radiation should undoubtedly be attributed to Jansky. In Dec. 1931, while investigating atmospherics on a wavelength of 15 m, he showed that radio waves of extraterrestrial origin were reaching the earth. Moreover, these waves showed a diurnal variation both in direction of arrival and intensity. Subsequent work showed that the periodicity of the noise was 23 hr 56 min, corresponding to the earth's rotation relative to the stars. Thus it was determined that the apparent source of the radio noise was fixed in space. During the next few years, Jansky continued his experiments and concluded that the area of maximum emission was in the direction of the galactic center and that the source appeared to be distributed along the Milky Way. He also con-

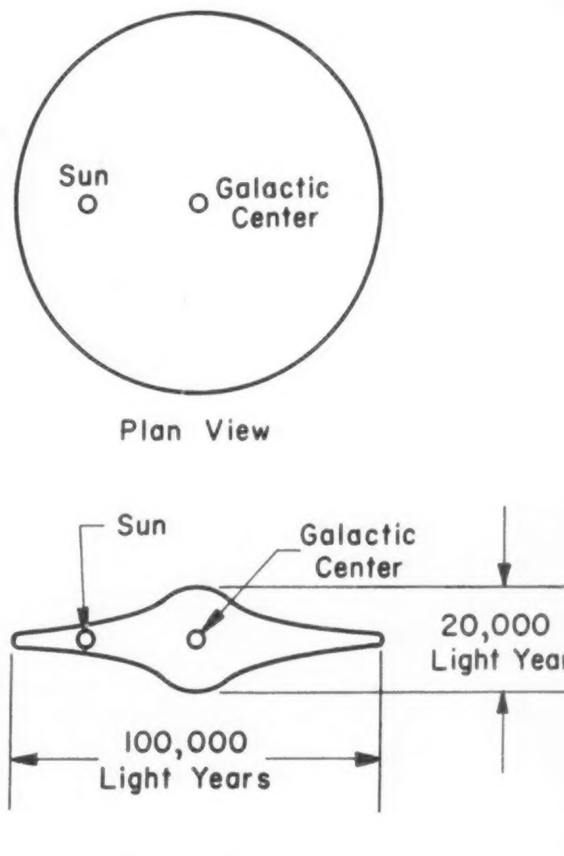

Fig. 6. Sketches illustrating our galaxy.

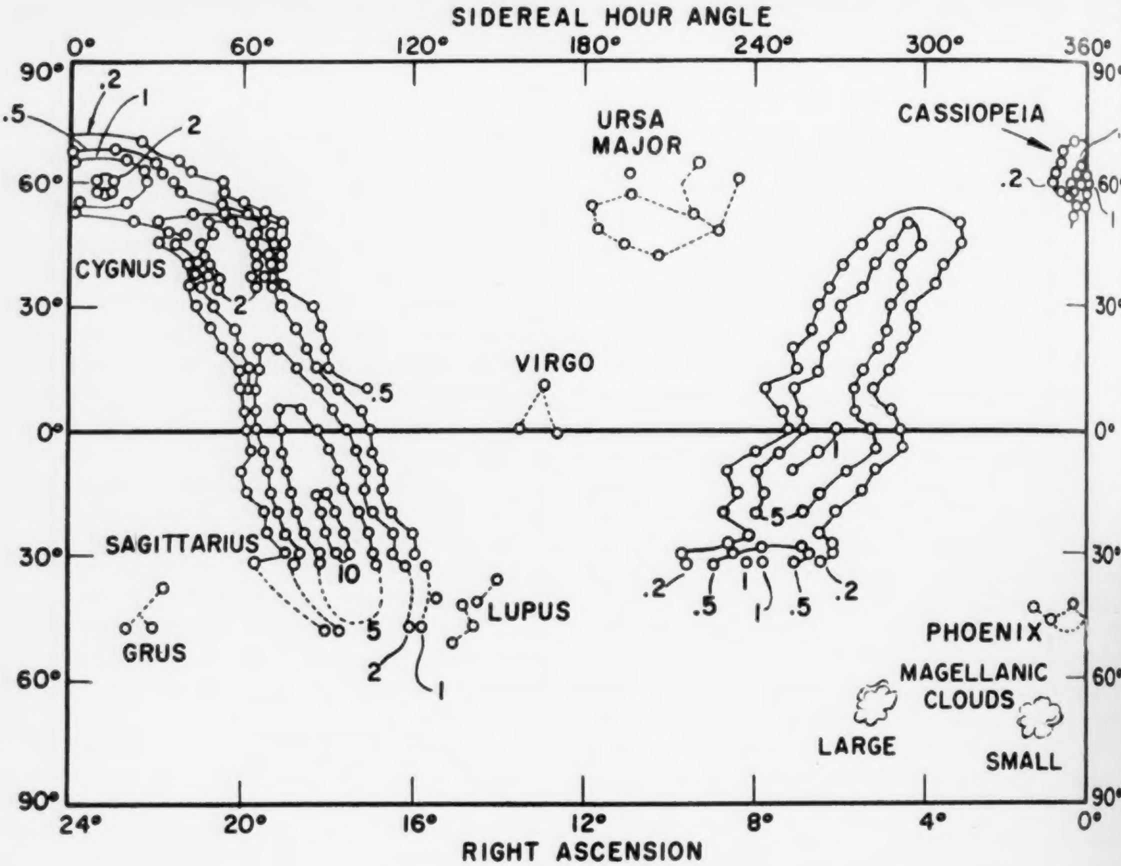

Fig. 7. Map showing the contours of cosmic radio noise observed by Grote Reber on 160 Mcy/sec. (Expressed in terms of 10^{-22} w per cm² cir-deg and per Mcy/sec band width.)

cluded that the noise originated either in the stars or in interstellar space and, in an attempt to verify his conclusions, he tried to detect radio emissions from the sun. However, these experiments failed.

Because of the lack of directivity in Jansky's antenna, it was left to Reber, during the years 1940–1944, to produce detailed plots of the distribution of the noise intensity along the Milky Way by using a wavelength of 1.85 m and an antenna beam width of about 12°. Figure 7 is a plot of his results and shows that the maximum intensity lies in the constellation Sagittarius toward the galactic center, with secondary peaks occurring in the constellations Cygnus and Cassiopeia.

By the use of even more highly directive antennas, Bolton and Stanley, working in Australia in 1948, showed that the angular diameter of the source in the constellation Cygnus was less than 8 min of arc and that it occurred in a region devoid of any outstanding visible objects. Soon afterward, Ryle and Smith discovered a similar very intense source of noise in the constellation Cassiopeia. By the summer of 1950, Australian workers had discovered 30 point sources in the Southern hemisphere and British workers, 50 in the Northern Hemisphere. Some of the latter are illustrated in Fig. 8. By the use of receivers with increased sensitivities and better antenna arrays, several hundred such point sources have now been charted.

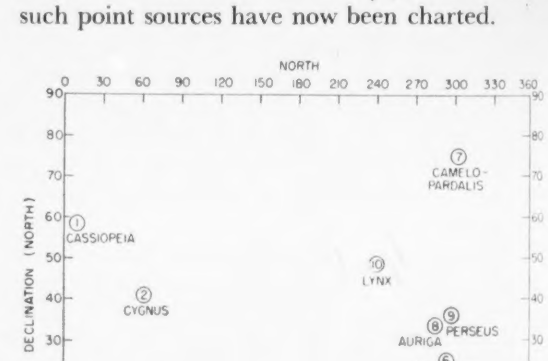

Fig. 8. Chart showing the positions of the ten most intense galactic sources in the Northern Hemisphere (listed in order of intensity).

Since the sun is a source of black-body radiation in the radio spectrum, it was first suggested that galactic noise was similar in origin. However, calculations showed that if all stars behaved in the same way as the sun, the aggregate intensity of the radio emission would be far too small by a factor of 10^9 to 10^{12} to account for the observed results. Moreover, the discovery of point sources of radio noise in regions with no significant visual stars made this idea untenable.

The mechanism of the emission of radio noise from such point sources is not yet understood. The existence of stars that are invisible through optical telescopes yet capable of strongly emitting radio waves has been postulated with the idea that there are about as many radio stars as there are visible ones in our galaxy (about 100,000 million of each).

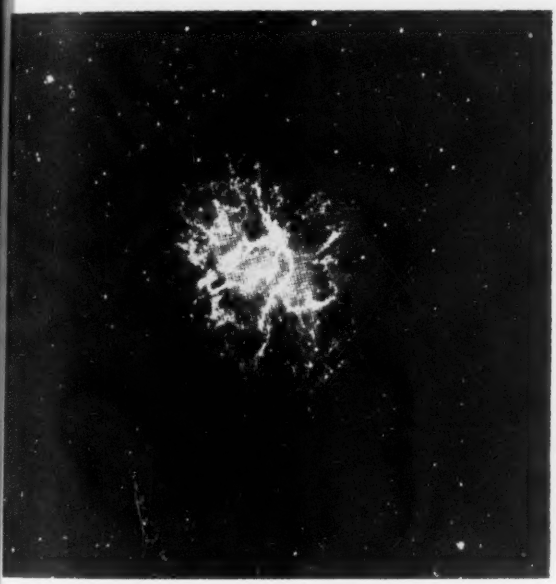

Fig. 9. The Crab Nebula.

Recently, however, the Crab Nebula in the constellation Taurus and the great nebula in the constellation Andromeda have also been identified as sources of radio noise. These nebulae appear as faint, hazy patches of light that cannot be resolved into stars and are illustrated in Figs. 9 and 10. The Crab Nebula is the gaseous remnant of a supernova, or exploded star, that disintegrated in 1054 when it could be seen in broad daylight.

One of the earliest attempts to account for galactic radiation of an interstellar nature was proposed by Reber in 1940. Interstellar space is believed to contain not only dust particles but also very rarefied hydrogen with a concentration of 1 or 2 atoms/cc. These atoms, according to not only Reber but also Henyey and Keenan, Townes, Van deHulst, and others, should emit radiation with a wavelength of

Fig. 10. The Andromeda Nebula.

approximately 21 cm. Such radiation was first detected on the night of 25 Mar. 1951 at Harvard University by the use of a highly sensitive receiver and a parabolic antenna system. On this occasion, the received signal came from the direction of the constellation Ophiuchus near the center of the Milky Way. Within a matter of months, confirmatory results had been obtained by workers in Australia and Holland.

Fig. 11. The Large Magellanic Cloud.

By measuring the exact frequency of the received hydrogen radiation and comparing it with the theoretical frequency that would be obtained in a laboratory, the so-called Doppler Shift in frequency is obtained, and from this the radial velocity of the hydrogen cloud may be determined relative to the earth. Dutch astronomers have detected as many as three or more hydrogen signals along a single line-of-sight into the galaxy. These results suggest the existence of several hydrogen clouds moving at different speeds relative to one another and to the earth.

In 1952, the first hydrogen clouds outside the Milky Way were detected. These occurred in the Large and Small Magellanic Clouds which appear to the naked eye as two detached portions of the Southern Milky Way. The Large Cloud is shown in Fig. 11 and its position is charted in Fig. 7. The two clouds were found to be revolving around each other and, at the same time, to be receding from our galaxy.

From the foregoing brief review of the present state of the science of radio astronomy, it is apparent that at least several basic problems, such as those related to the abnormal radiations from the sun and the origin of galactic radiation, still remain to be solved. However, by the time these questions are answered, scientists will have thought of many more questions that radio astronomy might answer, for it has been justifiably said that this science is in almost the same position that visual astronomy was when Galileo invented the optical telescope.

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New Research in Radio Astronomy

Some of the still unsolved mysteries of the universe—including the nature and origin of radio waves that come from outer space and the existence and location of stars that cannot be seen or photographed with even the largest telescopes—are to be explored by Australian scientists, using a giant "radio eye," or "dish," (see front cover) 250 ft in diameter and some 60 ft deep. Construction of the new "dish," technically called a parabolic reflector or receiving antenna, will be provided in part by a quarter-million dollar grant from the Carnegie Corporation of New York. About 3 years will be required to build and mount the reflector so that it can be tilted or rotated in any direction.

The "dish" was designed by the Radiophysics Laboratory of the Commonwealth Scientific and Industrial Research Organization under the direction of E. G. Bowen, a British-born physicist of international reputation, and his chief assistant, J. L. Pawsey. Work of the Radiophysics Laboratory, located on the campus of the University of Sydney, lies in the relatively new field of radio astronomy, now recognized as an essential aid in exploring the nature of the universe. The new "dish" may help to find and identify invisible radio stars and to investigate other suggested sources of radio waves, such as stellar explosions, the action of hydrogen gas in interstellar space, or collisions of galaxies.

Another "dish," comparable in size and power to the proposed Australian reflector, is now under construction at the University of Manchester, England. Ever since plans for the British "dish" were announced about 2 years ago, scientists have hoped for a similar unit that would make it possible to explore the stars in the Southern Hemisphere, particularly the Magellanic Clouds formation, with the same thoroughness that the Manchester equipment will make possible exploration of stars in the Northern Hemisphere. The proposed installation in Australia may be the answer.

Agriculture versus Chemistry in the Nutrition of Man

D. M. HEGSTED and FREDRICK J. STARE

Dr. Hegsted received his Ph.D. in biochemistry at the University of Wisconsin. After a short period in the research department of Abbott Laboratories, he went with Dr. Stare to Harvard University to establish the Department of Nutrition. He is associate professor of nutrition at Harvard and a member of the editorial staff of The Journal of Nutrition. *Dr. Stare graduated from the University of Wisconsin and received his M.D. from the University of Chicago. He has been head of the Department of Nutrition at Harvard since its beginning in 1942. He is also editor of* Nutrition Reviews.

THE American people have been extremely fortunate in that development of the United States occurred at a time when our population was small and when scientific knowledge and its application in technology were keeping well ahead of the population. Overpopulation with respect to food supplies never occurred in the United States, and it seems unlikely that it ever will occur.

Within recent years, the difficult and embarrassing problems associated with feeding densely populated areas that are producing inadequate food supplies have suddenly become our own concern. Many people believe that the exportation of technical assistance in an effort to help these areas is a major plank of our foreign policy, or at least that it should be. In any event, the changing political situation has made it clear that we can no longer ignore the food and population problems of areas that now have or will have situations so different from our own.

Our own personal interest in these problems has been sharpened by a recent visit by one of us to the Far East. The problems of Java are typical of many parts of the world where millions of people are living on small land areas, where nutritional disease is widespread, and where the solutions are not obvious. We have also been interested for several years in the nutrition problems of Peru. Although overpopulation is not a major problem of Peru, its nutrition problems are essentially typical of many tropical areas. The diets of the mass of the population are high in carbohydrate, low in protein, fat, and certain protective foods. Education and living standards are low, and health services are primitive in a large part of the country. Industry is only beginning to be developed. There is no reason to believe that these countries should or will go through the same evolution observed in this country. Rather, they should be able to take the knowledge and technical skills now available and put them directly into efficient use.

The accomplishments of nutrition science during the first half of the 20th century are well known. The principal effort has consisted of biological identification of the essential dietary factors, their isolation, studies of their function, and finally their chemical synthesis. Some 22 amino acids, 10 of which are probably essential for growth, a dozen or so vitamins, two or three fatty acids, and more than a dozen inorganic elements may now be included in the essential food elements. The marked success in growing animals for more than one generation upon diets that are highly purified leaves little doubt that we are approaching the end of this remarkably productive period. Much remains to be found out with regard to the functions of these nutrients, their interrelationships, the quantitative requirements of man and animals at different stages of growth and maturity, and so on. Nutrition will contribute a great deal to future medicine and public health, perhaps mostly in the field of the prevention of disease, particularly in geriatrics, the effects of diet upon the aging process. But the opportunities for success in identification of new growth factors or new essential nutrients are certainly becoming less. There is no reason to believe that the nutrition requirements of man are any more complicated than those of some of the common experimental animals, such as the chick and the monkey. Indeed, the more rapidly growing species usually have greater requirements for most vitamins and, if their requirements are no

more complicated in the qualitative sense, the effects of limited diets are more pronounced.

The realization that the techniques for isolation and synthesis are so well advanced that nutrients required in only microgram quantities are now available in pure form leads us to a reevaluation of the future. Nutrition science developed to a large extent in the agricultural colleges and experiment stations of this country. The reason, of course, is obvious to all of us; namely, that farmers, dairy and poultry producers, and beef herders saw that nutrition research paid off in real dividends. We now see indications that the product may turn to bite the hand that fed it. Several obvious examples present themselves. From the time that the Babcock test was developed, butterfat has been the basis on which milk is sold. Today the major problem of the dairy industry is the disposal of butterfat. Nutrition research has paid off to the consumer. Margarine, which as far as is known is equal to butterfat nutritionally, can now be produced much less expensively than butter fat. Furthermore, it seems quite clear that if the future should reveal nutritional advantages of butterfat, these findings would also soon be translated into an improved margarine. And margarine also always has the additional advantage of being a uniform product produced under controlled conditions that can be varied to suit consumer preferences.

Most of us now enjoy enriched white bread that is, as far as we know, the nutritional equal, when consumed with our ordinary diet, of whole-wheat bread. There is good evidence in man that the latter is inferior, because the higher content of fiber and phytic acid in whole-wheat bread interferes with our obtaining the slightly increased amounts of protein, calcium, and iron that it may contain. Thus, we obtain slightly less energy from the whole-wheat product, although we hasten to add that the enriched-white-bread versus whole-wheat-bread controversy has no practical importance in the nutrition problems of the United States. Under certain comparisons, particularly on the basis of protein quality, white bread suffers some disadvantage. But if it is ever shown that this is indeed a real nutritional hazard to the population, means are available to correct it by amino acid supplementation. With the help of the chemical factory, it should be possible to make a bread, or any other cereal product, with good quality protein. Such improvement of cereals with synthetic nutrients will probably be a continuing development. The resulting products will be of improved nutritive quality and will compete more favorably with foods of animal origin.

Although cereal improvement is important in this country, it is to the millions of people living in the peasant areas of the world and existing on one main cereal or grain crop, rice or corn, that the synthetic amino acids offer the greatest benefit. From these areas, we have heard much about a disease usually called kwashiorkor, or malignant malnutrition. It is found in infants and children after they have left the good protein of mothers' milk and have been on the protein of a single cereal for a period of some months. It is a common and serious disease that is frequently fatal; those who recover may be plagued with cirrhosis of the liver in later years. Yet this disease is completely preventable through better nutrition. Milk will prevent and treat it. So will the superior vegetable protein foods, such as mixtures of various legumes and nuts. But the areas of the world in which this disease is common have peasant methods of agriculture. These areas are frequently overpopulated and all available fertile land is needed to grow the basic crop of rice or corn. And it takes generations to change cultural and food patterns, particularly in a largely illiterate population. The proper nutrients, most likely amino acids, added to the rice or corn diet should be able to correct this nutritional deficiency almost overnight.

The fortification of bread and a few other products represents a realization by government that it has a responsibility for the nutritional health of the people. It is obvious that at present it is not full responsibility, and one is forced to ask at what stage government responsibility begins and ends. Enrichment of white bread is not simply making white bread equal to whole-wheat bread, since riboflavin is added in larger amounts than occur in whole-wheat bread. There is, of course, no reason to believe that nature endowed wheat with riboflavin in proportion to the needs of man. The addition of vitamin D to milk is another clear example of an addition being made according to human need. With these examples before us, what real justification is there for making Johnny eat his spinach? Spinach is a good source of vitamin A as carotene and contains some iron, vitamin C, folic acid, riboflavin, and other nutrients. All these are available at low cost and could undoubtedly be made available to Johnny in a food that he accepts more readily. Industries have been built on the nutritional benefits of a particular food and, as in the case of butter, may be greatly diminished by the results of further nutrition research. It seems clear that one of the results of nutrition research, considered in the broad sense, is to make any single food unessential in the diet.

The nutritionists themselves have perhaps been among those most reluctant to accept the results of their findings. Traditional nutrition education has grouped foods according to their nutrient content. The benefits of a varied diet are due, insofar as is known, to the various nutrients they contain. Nutrition practice today recommends foods even to poor groups when the nutrients they supply can be obtained less expensively as synthetic chemicals. Suppose the government decided that by the enrichment of a few basic foods any person who consumed a reasonable amount of common foods would not suffer from nutritional deficiency. Should we then emphasize nutrition education, or should people be encouraged to eat more or less what they wish with the knowledge that the foods they eat will supply adequate amounts of all the known nutrients? These are questions that deserve answers in the near future.

The argument may be carried somewhat further. The fields of investigation dealing with odors and flavors must certainly be expected to advance. The improvements in margarine may again be cited as an example. Who would be willing to wager that synthetic preparations equal to fruit juices in terms of flavor, appearance, and nutritional value will not soon be possible? Of course, any such preparation must compete financially, and we have no idea where and when this will be possible.

Synthetic chemicals have made serious inroads into the markets of wool, wood, insecticides, dyes, fertilizers, and practically every industry that at one time depended upon natural products. It would indeed require one to have little faith not to expect similar changes in the food industry. It may be noted that much of the past consumer resistance to "synthetics," particularly in the field of fabrics and partially in the field of food, has already been broken down.

It is necessary to point out that we do not believe that we or future generations will be eating concoctions of synthetic diets. All foods are consumed largely because they taste good and we like them. Everyone who can afford tasty foods will continue to eat them, and there is little reason to believe that synthetic beefsteak is just around the corner. However, there is doubt that they can continue to be promoted on a sound nutritional basis in competition with products nutritionally improved via the chemical industries. It seems only prudent that we recognize the problem that confronts us. Changes in agricultural production affect all of us, but changes are inherent in progress and they do occur.

The apparent competition between certain phases of agriculture and the chemical industry is only the competition inherent in a free economy, and there is little doubt that, in the long run, the consumer will benefit. Uneconomical production will be turned to more profitable areas. The food industry that attempts to grow, or even exist, on its past record will not be with us long. A constant search for nutritional improvement of its food products must be carried on, and much of the nutritional improvement of the future will come from nutrients purchased from the chemical factory. The chemical industry has made much headway in favorably supplementing the food products of agriculture. Much more will be forthcoming. The farmer and the chemist together can do a better job of solving the problems of global nutrition than either one alone.

Species That Feed Mankind

The paper by Hegsted and Stare, "Agriculture versus chemistry in the nutrition of man," was presented at the Boston meeting of the AAAS, December 1953, as a part of the symposium *Species that feed mankind*. This symposium was arranged for the meeting by Paul C. Mangelsdorf of Harvard University and M. R. Irwin of the University of Wisconsin.

Three papers that were presented as part of the same program have already appeared in *Science*: O. T. Bonnet, "The inflorescences of maize" (16 July); Marcus M. Rhoades, "Chromosomes, mutations, and cytoplasm in maize" (23 July); L. A. Maynard, "Animal species that feed mankind: the role of nutrition" (30 July). Subsequent issues of *The Scientific Monthly* will carry the remaining papers in the series.

Bibliographies of Eminent Scientists

WAYNE DENNIS

Dr. Dennis received his training at Marietta College and Clark University. He taught at Michigan State College, the University of Virginia, Louisiana State University, and the University of Pittsburgh and is now professor of psychology and chairman of the Department of Psychology, Brooklyn College. Dr. Dennis is editor of Psychological Bulletin *and has published numerous articles in psychological journals.*

CONVERSATIONS with scientists indicate that they frequently assume that the work for which a man of science is best known constitutes, quantitatively as well as qualitatively, a considerable part of his scientific output. Probably this is due to the fact that the name of a scientist, even a well-known one, is usually associated with one or, at most, with a few outstanding discoveries or theories. For this reason, persons are prone to assume that he has done little else. In regard to the majority of eminent scientists, this is a false conclusion. This article is devoted to presenting evidence that, in most instances, eminent scientists have published an unusual number of scientific articles, monographs, and books. It also presents data that indicate that the greater the number of a scientist's publications, the higher his scientific reputation is likely to be.

Two groups of scientists, beginning with a group that has been distinguished by election to the National Academy of Sciences, are considered here. Certainly not all first-rate American scientists receive this honor, nor are all who are elected to the National Academy immortal, but as a group, the members of this body are highly competent. What is their record in regard to productivity?

This is easily ascertained, because when a member of the National Academy dies, a biographical sketch and a bibliography appear in the *Biographical Memoirs* of the Academy. A bibliographic count was made for each man whose bibliography appeared in the *Biographical Memoirs* between 1943 and 1952, inclusive, and who reached age 70 before his death. There were 41 such men. The choice of septogenarians was dictated by the needs of another study (*1*). Persons dying at an earlier age average a lower total productivity, but my findings would not be much changed by the inclusion of persons with shorter life-spans. Persons whose bibliographies consisted primarily of patents were omitted, because I did not wish to combine patents and publications.

The bibliographies appearing in the *Biographical Memoirs* of the National Academy of Sciences have not been compiled in a uniform manner. Some are apparently quite complete. Others are "selected" bibliographies. Few of them are limited to strictly scientific publications. Most bibliographies include public addresses and semipopular writings as well as scientific articles. Thus, these bibliographies will satisfy neither the person who wants *complete* lists of publications nor the individual who wishes sources that are limited to scientific contributions. On the whole, they lie somewhere between these two extremes.

According to the bibliographies as published, these 41 men were responsible for a total of 8332 works. This means an average of 203 per person. The highest record was 768 items, the lowest 27. Eleven had 300 or more publications each. Only 15 had fewer than 100 publications each. The median number was 145.

There are no data at hand on other scientists living at the same time as these men. I believe, however, that few will doubt that among the generality of anatomists, botanists, chemists, and so forth, the records summarized in foregoing paragraphs are unusual ones. Indeed, I would hazard the guess that not more than 10 percent of the persons listed in *American Men of Science* equal the record of the least productive member of the Academy (27 publications).

No doubt various fields of science differ in the extent to which they lend themselves to high rates

of publication. My data are too limited to justify a detailed comparison of scientific specialties. However, I wish to note that the upper ranges of various groups are quite similar. The 11 men in the National Academy who had 300 or more publications came from such diverse fields as anthropology, astronomy, biochemistry, chemistry, engineering, genetics, geology, medicine, physics, and zoology. These men, in the order of the number of their publications, are as follows:

P. A. T. Levene	Biochemistry	768
C. H. Merriam	Zoology	626
L. J. Stegneger	Zoology	499
W. M. Davis	Geology	477
C. Barus	Physics	420
A. E. Kennelly	Engineering	362
T. B. Johnson	Chemistry	358
A. Hrdlicka	Anthropology	340
W. W. Campell	Astronomy	330
H. Cushing	Medicine	306
C. D. Davenport	Genetics	303

For a second group of eminent scientists, I chose those persons whom Raskin (*2*) selected as the 25 most eminent scientists of the 19th century on the basis of the space devoted to them in several encyclopedias and dictionaries of biography. The bibliography of each man has been determined by referring to the Royal Society of London's *Catalog of Scientific Literature, 1800–1900*. Complete records could be obtained for only 19 men, because six began publication before 1800. These men and their respective bibliographies are as follows:

Liebig	307
Bertholet	236
Pasteur	172
Faraday	161
Poisson	158
Agassiz	153
Herschel (J. F. W.)	151
Humboldt	142
Gay-Lussac	134
Gauss	123
Kelvin	114
Maxwell	90
Joule	89
Davy	86
Helmholtz	86
Lyell	76
Hamilton	71
Darwin	61
Riemann	19

At first glance it might seem that these eminent 19th century scientists were less productive than the members of the National Academy of Sciences for whom data are given in foregoing paragraphs. However, such a conclusion is unwarranted. All 19 of the eminent 19th century scientists are listed, whereas, from the 41 members of the National Academy, only the top 11 are listed by name. It should be noted that the bibliographies were obtained by different methods and, hence, are not strictly comparable. The bibliographies obtained from the *Catalog of Scientific Literature, 1800–1900* contain only papers appearing in scientific periodicals, whereas the *Biographical Memoirs* of the National Academy often contain, in addition to scientific papers, some popular writings, public addresses, book reviews, and so forth. A further difference to be noted is that the members of the National Academy whom I chose for study consisted only of those who lived to age 70 or beyond. Raskin's group, on the other hand, was not selected on the basis of age. Because of these differences, the reader should not draw any conclusion with regard to comparisons between these two groups.

In the case of the scientists of the 19th century, it is possible to make a comparison between the number of publications of the most eminent and the scientific output of the general run of their contemporaries. In order to obtain data on the productivity of 19th century scientists in general, I chose what approximates a 1-percent sample of all persons whose names appear in the *Catalog of Scientific Literature, 1800–1900*.

My first step was to select a random sample of names from the first section of the catalog, which covers the period 1800–1863. The record of publication of each person chosen was then checked for the period 1863–1900 as well as for the period up to 1863. Each record may, therefore, be assumed to cover the period from the date of a person's first publication, as listed in the catalog, to the end or nearly to the end of his life-span.

I obtained my sample by selecting the names on page 100 and on every 100th page thereafter in volumes covering the period 1800–1863. In an attempt to rule out persons who may have begun to publish before 1800 and whose record of early publication would therefore be incomplete, I omitted the names of those whose first publication appeared before 1810. If a person had not published in the decade between 1800 and 1810, I assumed that he had not published prior to 1800 and that his period of publication belonged entirely in the 19th century. While the assumption no doubt involves some errors, I do not believe these errors seriously affect the findings to be reported. All volumes of the catalog were then searched to obtain a total record of publications for each man. In brief, I believe I have a complete list of periodical publications for a 1-percent sample of the scientists who began to publish between 1810 and 1863. The number of names obtained by this method was 208.

The range was from one to 458 publications. Thirty percent of this group had only one publication each. Half had fewer than seven publications. Riemann, the least productive of Raskin's cases, who had 19 publications, was equaled in quantity by only 25 percent of 19th century scientists. Had he not died at age 40, he would undoubtedly have produced more. Aside from Riemann, the remainder of the eminent scientists belong in the top 10 percent in terms of productivity.

My sample of 19th century scientists was checked for inclusion in the *Encyclopaedia Britannica,* an honor not easily achieved. Of the men of greatest productivity (highest decile, 21 persons), nine, or approximately one-half, appear in the *Encyclopaedia Britannica*. Of the remaining 187, only six are so listed. Some of the latter are clearly listed for their distinction in other fields rather than because of their scientific contributions.

On the basis of these facts, it is clear that there is a definite relationship between productivity and eminence in science. However, it is also clear that the two are by no means identical. We can illustrate this lack of identity by referring to two scientists not included in the preceding groups, who occupy places almost at the two extremes of productivity. Near the lower end of the distribution of productivity is Mendel. Mendel, in terms of the criterions employed by Raskin, did not win a place within the first 25 scientists of the 19th century, but he probably came near to it. He is mentioned in every history of biological science. Yet he published only seven scientific papers. It should be noted that the cessation of the scientific work of Mendel seems to have been due to the pressure of other duties. Near the other extreme of productivity is John Edward Gray, an English naturalist. The catalog of the Royal Society lists 883 publications for him. Doubtless these had some value, yet his name does not appear in any history of biology that I have examined, nor does it appear in the *Encyclopaedia Britannica.* Apparently he is a relatively unimportant figure.

These two men are exceptions to our general findings and doubtless other exceptions can be found. But such extreme exceptions to the general trends are rare. Let us examine my statistics in this connection. On the basis of my 1-percent sample of scientists of the 19th century, I estimate that the Royal Society catalog lists the names of 13,080 persons who published seven or fewer scientific papers. Of these persons, I do not know of any others who have achieved the fame of Mendel. It is clear that persons who publish seven or fewer papers are not likely to achieve distinction, although Mendel's case shows that it can be done.

With those of high productivity, the situation is very different. Among 19th century scientists who produced 50 or more publications (they are 10 percent of the total group), almost one-half can be found in the *Encyclopaedia Britannica.* Of those who published more than 140 papers (the top 5 percent), 70 percent appear in the *Encyclopaedia Britannica.* Among highly productive people, anomalies such as John Edward Gray exist, but they are most unusual. Indeed, I have found no other relatively undistinguished person who was so productive as Gray.

Accepting the fact that eminent scientists are usually highly productive, how is one to explain this relationship? I do not propose to offer a definitive explanation, but I will hazard some suggestions. Whatever else is required to achieve eminence in science, sustained effort is one prerequisite. No matter how able the man, eminence does not come easily. Anne Roe's investigations (*3*), as well as my own (*1, 4*), indicate that the eminent scientist works hard at being a scientist. The same drives that enable a scientist to do the things for which he is well known probably cause him to perform many other pieces of scientific work. He does not quit when he has produced his magnum opus; he does not quit when he achieves recognition; he does not quit when he retires from his position. An examination of the productivity of eminent scientists throughout their life-spans (*1*) shows that an extraordinary level of activity is maintained to age 70 and beyond. Hard work is not the whole story, but the scientists who achieve recognition are industrious, and, as a result, their bibliographies are long.

What about the other facet of the relationship, namely, that the more productive the man, the more likely he is to be eminent? Certainly a man does not attain distinction by sheer quantity of output. My suggestion is a tentative and partial one, but I submit that the correlation between fame and fecundity may be understood in part in terms of the proposition that the greater the number of pieces of scientific work done by a given man, the greater the likelihood that one or more of them will prove to be important. No one can foresee perfectly whether a given scientific project will be a fruitful one. While scientists undoubtedly differ in their ability to select areas that will lead to "pay dirt," whether they find it or not depends in part upon chance. Other things being equal, the greater the number of researches, the greater the likelihood of making an important discovery that will make the finder famous.

I am quite aware that these "explanations" are oversimplified and inadequate. I offer them only to

indicate possible future directions of search. My primary purpose in this article has not been to explain scientific discovery. Rather my purpose has been merely to show that eminent scientists usually are highly productive, and that the likelihood of achieving a certain degree of eminence increases with the number of publications. In science, quantity and quality are correlated, although they are not identical.

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Books Reviewed in SCIENCE

2 July

Microwave Theory and Techniques, H. J. Reich *et al.* (Van Nostrand). Reviewed by Anthony B. Giordano.

Australian and New Zealand Botany, John McLuckie and H. S. McKee (Associated General Publ.). Reviewed by Harriet B. Creighton.

Protozoaires: Rhizopodes, Actinopodes, Sporozoaires, Cnidosporidies, Pierre-P. Grassé, Ed. (Masson). Reviewed by William Balamuth.

Renal Function, Stanley E. Bradley, Ed. (Josiah Macy, Jr. Foundation). Reviewed by A. V. Wolf.

British Veterinary Codex 1953, Council of the Pharmaceutical Society of Great Britain (Pharmaceutical Press). Reviewed by John E. Martin.

Microwave Lenses, J. Brown (Wiley; Methuen). Reviewed by H. Y. Fan.

Hydrocarbons from Petroleum, Frederick D. Rossini, Beveridge J. Mair, and Anton J. Strieff (Reinhold). Reviewed by Gustav Egloff.

9 July

Cell Chemistry, Dean Burk, Ed. (Elsevier). Reviewed by Sidney P. Colowick.

Signal, Noise and Resolution in Nuclear Counter Amplifiers, A. B. Gillespie (McGraw-Hill; Pergamon Press). Reviewed by W. C. Elmore.

A Bibliography of the Research in Tissue Culture: 1884 to 1950, vols. I and II, Margaret R. Murray and Gertrude Kopech (Academic Press). Reviewed by Philip R. White.

Manuel de Paléontologie Animale, Léon Moret (Masson). Reviewed by Joel W. Hedgpeth.

Electrostatique et Magnétostatique, Emile Durand (Masson). Reviewed by William Fuller Brown, Jr.

The Furans, A. P. Dunlop and F. N. Peters (Reinhold). Reviewed by George F. Wright.

16 July

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The Chemical Structure of Proteins, G. E. W. Wolstenholme and Margaret P. Cameron, Eds. (Little, Brown). Reviewed by Milton Levy.

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Notions Elémentaires de Chimie Générale, Paul Pascal (Masson). Reviewed by Beverly L. Clarke.

Handbook of Freshwater Fishery Biology with the First Supplement, Kenneth D. Carlander (Brown). Reviewed by Karl F. Lagler.

23 July

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30 July

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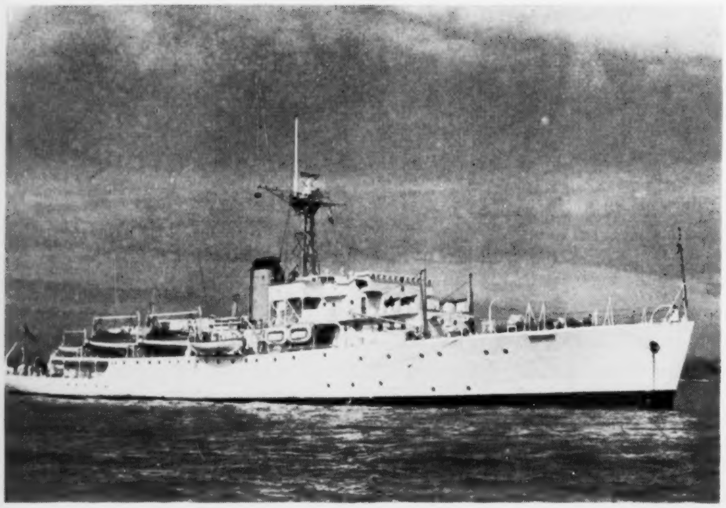
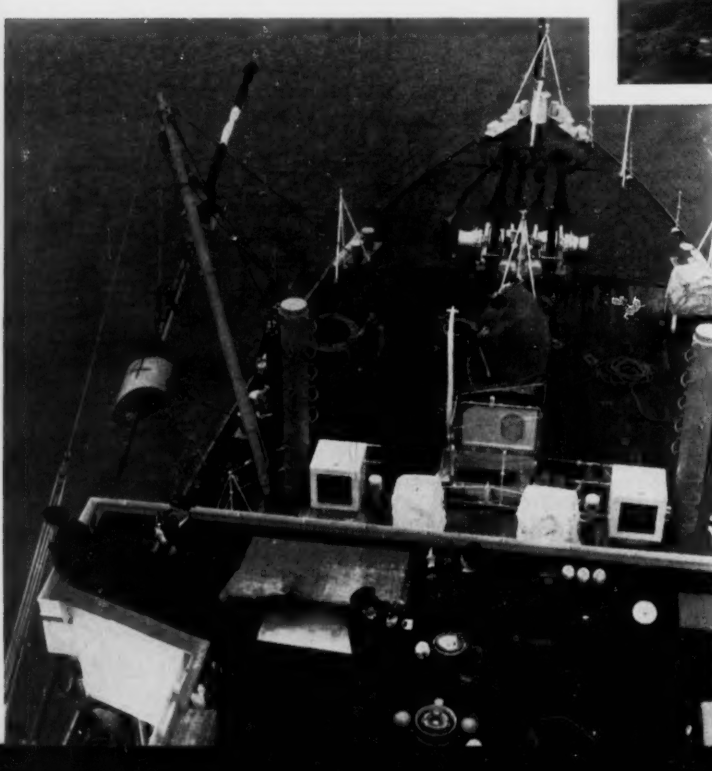


For 8 out of 12 months, the survey ships of Britain's Hydrographic Department of the Royal Navy are out at sea collecting data about the ever-changing sea bed. Some of these ships, like H.M.S. "Cook" (left), the latest of eight commissioned, operate in home waters. Others are stationed in the Mediterranean, Red Sea, Persian Gulf, and Far Eastern waters. H.M.S. "Challenger," a special oceanographic vessel, holds the record of obtaining the deepest specimen taken from the bottom of the ocean—a reddish clay brought up from 5744 fathoms.

Surveys of deep waters are covered by the ships; surveying motorboats, equipped with echo-sounding apparatus and radiotelephone, are used in the shallows. A boat-sounding party from H.M.S. "Cook" (right) is seen fixing the motorboat's position with sextants. This is done at frequent intervals as the boat proceeds along a line.



When the need for a resurvey of an area arises, the Hydrographer of the Navy at the Admiralty issues "Hydrographic instructions" to the ship selected for the task. If no triangulation points exist in the operational area, one is created by the ship's company. Here, for instance, a floating beacon is being hoisted on board H.M.S. "Cook." The floating beacon, an anchored buoy with a flagpole, serves as a triangulation point from which the positions of soundings taken may be fixed.

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If there are no ordnance survey marks, such as a church spire, a pole, or conspicuous tree, on shore in the area to be charted, a party is sent from the survey ship to establish one. At the right, an officer and two of his crew from H.M.S. "Cook" are seen hoisting the specially erected flag used as a reference (ordnance survey) mark. During the months when the surveying ships are being refitted, officers are loaned convenient offices in the dock-yard where they produce "fair charts" from the operational ones. [All photographs by courtesy of British Information Services.]

From the sounding boards, used in the surveying motorboats, the information is transferred to charts in the chart room (left) aboard the surveying ship. More important discoveries are transmitted at once to the Hydrographer of the Navy; the remaining data are sent to the Chart Branch of the Hydrographic Department at Cricklewood, London.

At Crinklewood a small naval staff and a large number of civilian hydrographic officers collate the information received, issue notices, light lists, and sailing directions to mariners, and keep up-to-date records of the charted seas. Some half-million documents are stored in the archives. The oldest, the *Coasting Pilot of Great Britain,* was compiled by Captain Grenville Collins during the reign of Charles II. Admiralty charts were first made available to the Mercantile Marine and the public in 1823.

Her Secrets

Reviews of Books and Recordings

The Challenge of Man's Future. Harrison Brown. Viking Press, New York, 1954. xii + 290 pp. Illus. $3.75.

THE necessity of a planned and stable world population is argued in Brown's book. Extrapolating from present knowledge of populations and food, energy and mineral resources, it discusses various biological and technical problems associated with this stability. The gravity and difficulty of these problems are very clearly expounded.

The author briefly sketches the evolution of man through the agricultural and industrial ages, emphasizing the interdependence of world populations, food, energy, and materials. The chapter on "Vital statistics" contains various demographic information on our changing world society, including the influence of medical sciences on the growth and aging of populations. The chapter on food reviews the principal sources of man's food and estimates the foreseeable potentiality on the earth's surface. The ever-increasing demand for energy, along with estimates of fossil, solar, and nuclear energy, is discussed in another chapter. A chapter called "Things" describes other material resources needed by man, with estimates of their future supply and demand.

Many sections of the book, especially the last chapter, "Patterns of the future," are what the dust jacket describes as "startling and . . . hair raising." Brown sees three possible future patterns of life. The first and most likely, according to him, is a reversion to an agrarian existence. The second is a completely controlled, collectivized industrial society. The third possibility, that of a world-wide, free industrial society in which men can live in reasonable harmony with their environment, is, in his opinion, difficult to achieve, difficult to maintain, and could not last for long.

Because of Malthus' insufficient knowledge of the potentialities of technological development, the author justly criticizes Malthus' prediction of disaster for man. However, with 150 years more of rich technical development behind him, Harrison Brown appears to be making the same error, for nowhere in the book is any confidence shown in advancements through basic research. To be sure, in his extrapolations he allows for improvements in efficiency through engineering, but nowhere does he allow for the possible discovery of new sources comparable to the discovery of the utilization of fossil, solar, or nuclear fuels. Nowhere does he express the faith that the same intelligence that discovered our present fuels could also find energy sources in the sands, seas, and air around us.

Although Brown is most convincing in describing the truly great gravity of the material problems in the future for man, he lightly passes over other attributes of man in emphasizing man's biological appetites. The intellectual and spiritual appetites of man also present grave and challenging problems for man's future. Brown's equations of population, food, and energy do not allow for these intangible qualities of mind and spirit, and it is this omission that raises questions concerning the merit of the author's implied recommendations to the government on foreign policy, to the clergy on their influence on demography, and to young lovers on love.

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A History of the Theories of Aether and Electricity. Sir Edmund Whittaker. Philosophical Library, New York, 1954. xi + 319 pp. $8.75.

FROM 1900 to the late 1920's physics underwent a revolution perhaps more profound and certainly more violent than had been witnessed since the days of Galileo and Newton. The historian of this period has a unique opportunity. Not only does he deal with a time of dazzling achievements, but in the swift progress of science he can already begin to assess those achievements in historical perspective while many of the participants and eyewitnesses are still available to him. Indeed, Sir Edmund Whittaker has himself been in the thick of the battle. An outstanding mathematician and theoretical physicist, he has been the teacher of two generations of physicists through his treatises (for example, *Analytical Mechanics*). Now an octogenarian, he has shown in his recent writings the same vigor, excellence of style, and historical interest that distinguished the first volume of the present work (published 1904, revised 1952), which dealt with pre-1900 physics.

This second volume deals with the early nuclear atom, the theories of special and general relativity, quantum theory, matrix mechanics and wave mechanics. The book serves three purposes. Primarily it is a *physics textbook,* a 310-page review in which the main mathematical steps are briefly given on a level equivalent to that in graduate physics courses. Second, it is a *chronology* of recent physics. About 1000 dated references testify to Whittaker's thoroughness and dedication to his gigantic task. Finally, the book attempts to provide a *historical discussion* of main developments.

As was perhaps inevitable within the space limitations, this last is the less successful part. For example, Lindsay and Margenau's *Foundations of Physics* had more valuable material. Moreover, the book suffers from some interesting biases, as is demonstrated most clearly in the chapter on the theory of special relativity, which is entitled "The relativity theory of Poincaré and Lorentz." Einstein's historic first paper on relativity is introduced as follows:

> In the autumn [of 1905] . . . Einstein published a paper which set forth the relativity theory of Poincaré and Lorentz with some amplifications, and which attracted much attention.

This is not a studied understatement, but represents

Whittaker's evaluation. Of the 50 pages in the chapter, barely three discuss Einstein's work; the phrase "Einstein's special theory of relativity" is never used in the book—it simply is Poincaré-Lorentz relativity.

More seriously, one reads "The study of relativistic dynamics was begun in 1906" by Planck, which ignores the equations of motion in Einstein's 1905 paper. Similarly, the famous equation for kinetic energy becomes "Planck's Equation of 1906." Regarding $E = mc^2$, we read that Einstein

> . . . suggested the general conclusion, in agreement with Poincaré, that the mass of a body is a measure of its energy content.

Again, the crucial point is omitted: Einstein's predecessors had in mind the inertia of electromagnetic energy, whereas he generalized the concept to include other forms of energy. In short, one misses a simple statement of facts which now are beyond controversy: that it was Einstein's insistence on the fundamental importance of the invariance of *all* physical principles that helped to rebuild physical science in a new and fruitful way; that it was his work of 1905 that catalyzed the conceptual merger of space and time, and of mass and energy; and that it was his influence, both scientifically and philosophically, that shaped in large part the mode of contemporary scientific thinking.

Whittaker may have two reasons for underemphasizing Einstein's special relativity. He rightly wishes to remind us of the background of famous theories that are popularly supposed to have sprung full-grown from the pen of one man. But the other, more disturbing reason is perhaps an unwillingness to note the full effect that Einstein's work had after 1905 on the conception of the aether. In his previous volume, Whittaker wrote

> . . . toward the close of the nineteenth century, chiefly under the influence of Larmor, it came to be generally recognized that the aether is an immaterial medium, *sui generis*. . . .

This conception is evidently carried over into the new book; the aether still lurks behind the scene in its function to make space, if not *material*, at least *physically real*. But precisely here lies the philosophical difference between Lorentz and Einstein. Before Einstein, the Lorentz transformations were mathematical manipulations superposed on a classical theory of mechanics and electromagnetism in which an aether of some sort was implicit. Einstein, on the other hand, had the audacity to demand that physics be built around these transformations as core elements, sacrificing the physical meaning of aether, of the FitzGerald contraction, and of the absolutes of space, time, velocity, and simultaneity.

From this conception, Lorentz and his followers recoiled. As late as 1914, Lorentz said

> As far as concerns the lecturer, he finds a certain satisfaction in other conceptions, that aether possesses at least some substantiality, that space and time can be sharply separated, that one can talk of simultaneity without further specification.

The verdict of history has been against Lorentz. To omit not only the verdict but even a discussion of the problem itself is to leave an important gap in the history of physics and especially in the work entitled *A History of the Theories of Aether and Electricity*.

The author has promised a third volume, which will bring the development of physics to 1950. One may hope it will include discussions of Eddington's *fundamental theory* and of the British cosmological theories. In spite of the reservations mentioned, the three volumes together will surely establish themselves as a unique survey of modern physics.

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Two Roads to Truth. A Basis for Unity under the Great Tradition. Edmund W. Sinnott. Viking Press, New York, 1953. xii + 241 pp. $3.50.

ALL thoughtful persons realize that our great Western tradition of culture has two distinct, divergent roots, the classic root of Rome and Greece and the religious root that traces back through the early Christian Church to the Hebrews. Strangely blended in the Renaissance, these two currents of thought have often clashed in such outright combat that the unity of Western culture has been imperiled. Today, as critically as in the 18th or 19th centuries, although in new guise, the cleavage threatens to become catastrophic. On the one hand are those who exalt "religion, the tradition of the spirit," as the guiding force in life; on the other, the devotees of the relatively young and increasingly powerful force of modern "science, the tradition of reason." It is this breach that alarms E. W. Sinnott, and he calls on us to heal it.

The book is a clear analysis of the problem and an eloquent plea for its solution. Sinnott, at least for himself, has found a way to advance toward truth by both roads, in the conviction that ultimate truth is one, that true religion and true science cannot conflict, and that each can speak in areas of life where the other lacks competence. He calls for greater tolerance and a mutual appreciation of the contributions made to life by both science and religion. ". . . the Great Tradition . . . maintains that both Reason and Spiritual Insight are valid means for reaching truth. . . . Man is a creature of both mind and spirit, and destined to be the battleground between them. . . . Reason and spirit are the pillars that support our Great Tradition. They must both be strong, but neither can be so without the other's help. Between them they hold up the hopes of man today as he strives to fulfill his splendid destiny."

In spite of fundamental sympathy for this position, and in spite of Sinnott's courageous discussion of such prodigious problems as determinism and freedom, mind and body, and the existence of cosmic purpose, one may nevertheless confess a doubt that the ultimate conflict will be allayed in most minds. More than a century ago, and well before the Theory of Evolution shattered old faiths, religious and scientific alike, the scientists who believed in Divine Providence were already face

to face with the dilemma. The vaster the realm of natural law in time and space, the greater might one conceive the Power and Wisdom of Providence to be, but so much the less any need for His direct intervention in the management of affairs. A First Cause, *Yes*; but a present, active Deity, *No*. Yet the latter is what religious faith still means to most people who cling to the religious pillar of our Great Tradition. A God immanent in His Creation, as mind is united with body, may be a belief that a scientist can readily accept; but it is the personal God that is the crux of the Christian tradition.

When, moreover, one considers the absurdities that have been believed in times past by religious leaders who fully thought themselves to be guided in belief by virtue of spiritual insight, it is hard to see how any faith in spiritual insight as a road to truth is to be maintained. No doubt real spiritual insight exists, but how distinguish it from imposture without and prejudice within? Is it not likely that conscience, the instrument of spiritual insight, is itself a product of man's evolution, an essential piece of the fabric whereby natural selection has made of man a social creature? If man's ethics and conscience, his whole moral and spiritual nature, are products of evolution, like the rest of him, they may indeed afford a reliable guide to what was good under the conditions of the past wherein they were evolved; but now, conditions being so vastly different than in prehistoric times, only reason remains as guide for present and future. Reason—and a profound faith.

BENTLEY GLASS
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Europe and America Since 1492. Geoffrey Brunn and Henry Steele Commager. Houghton Mifflin, Boston, 1954. xiv + 907 pp. Illus. + maps. $6.75.

A NEW textbook by authors of distinction does not guarantee a work of high quality. It too often is merely a re-edited compilation from other writings by the same authors, with little that is original except the title and the pattern of organization. Not so with this book. Most of its narrative and descriptive subject matter is familiar historic information, but it is put in a new framework that suggests new implications. Novel relationships of facts make possible a different emphasis and lead to some original conclusions. This volume stimulates a new appreciation of the role of the New World, and especially of the United States, in the history of Western cultural progress.

Even without the urgency of our recently enhanced "leadership" in world affairs, it is time that the sharp distinction between European and American history is bridged more specifically than has been traditional in our college history textbooks. To write of the development of American politics and culture highlighted against a background of European history is, of course, not new, but this book does more than that. The central theme is found in the foreword, ". . . neither America nor Europe can properly be studied without perpetual awareness that both are parts of a global community." The authors make this "awareness" real, vivid, and dramatic.

Brief, in relation to its scope, but basic, in its selection of materials and its emphasis, this book is a masterpiece of condensation and integration. This quality makes it hard going in spots for the casual reader and perhaps for the average college undergraduate, but interest is sustained throughout. Attention to the impact of events upon ideas (such as the effect of machinery upon 19th century thought, for example) pervades the whole narrative and gives spirit and significance to a remarkably comprehensive historical survey. A political framework serves as a support for cultural history rather than overshadowing it. Like any capable child of substantial parents, the United States of America owes deep debt to the cultural traditions of the European continent and the British Isles, but it has also forged beyond those traditions, has modified and added to them. The extent to which this is true has not been better indicated than it is in this volume. The illustrations and maps are meaningful, the format is excellent, and a carefully selected bibliography, arranged topically, adds greatly to the usefulness of the book.

MARK M. HEALD
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Symbolic Logic. Irving M. Copi. Macmillan, New York, 1954. xiii + 355 pp. $5.

THIS is a textbook designed for a one-year course, as a conservative but modern introduction to symbolic logic, for undergraduate and graduate students. Most of the theory is illustrated by numerous examples and supplemented by adequate exercises. The first five of these nine chapters should come within the understanding of any industrious student. The drill in translation and formal manipulation is particularly effective, save in case of truth tables. (The author could well have followed the excellent scheme of Quine in his *Mathematical Logic*, 1940.) The description of the logistic method is excellent. Although relations are discussed extensively, classes are relegated to a minor appendix. Gödel's theorem is covered in half a sentence. Multiple-valued logics are dismissed briefly. Various elementary systems are treated in some detail, including the Hilbert-Ackermann system, Lukasiewicz's notation, the Peirce-Sheffer-Nicod system, and a new system of first-order functions. The discussion of "deductive systems," is detailed and free of common errors. The topic of induction in natural science is dismissed as outside the scope of this text. The treatment of metalanguages, syntactic and semantic, is admirable. Nothing is said concerning modal logic, extension and intension, connotation and denotation, or existence, but the meaning of the decision problem is made clear. Although the author describes the traditional "square of opposition," the old-school logician will find here scant reference to the historic logical fallacies still rampant in daily disputation. The author seems to drop his avowed definition of "meaning" for "proposition" (p. 3), in favor

of "expression" (see p. 68), or "sequence of words of English" (p. 183). The ambiguous word "any" is used extensively, without discussion, (on p. 76). But one cannot replace "any" by "all" or "every" in say exercise 4, page 95, "If any diamonds are large. . . ." This reviewer does not accept the proffered symbolization with "($\exists x$)," in the middle of pages 93 and 94. But these may be rare misprints.

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Dynamic and Abnormal Psychology. W. S. Taylor. American Book, New York, 1954. xiv + 658 pp. $5.50.

THIS book is dated. The problems and the concepts set up are, for the most part, recognizable as those of a quarter-century ago. The historical treatment of psychoanalysis brings it down only to W. A. White. The "dynamic psychology" part of the book includes listings of all sorts of urges presented descriptively in a manner reminiscent of Thorndike and with no recognition of latter-day studies of motivation (such as need-reduction), experimental or other. The treatment of action starts in part from the notion of a resuscitated ideo-motor action.

The "abnormal psychology" chapters include those entitled "Association," "The subconscious," and "Suggestion"—all reminding one of the abnormal psychology of the French Janet school and of Morton Prince. There is a superficial handling of a few modern-sounding topics, such as "reactions to stress," and much too great reliance upon other college-level textbooks (as J. J. B. Morgan's).

In style, the book is decidedly readable, at least the many concrete examples, ranging in length from a single sentence to many paragraphs, which crowd the pages. In a way, then, here is a perfect mine of interesting incidents and illustrations; if one reads for them rather than for any clear organization or for any acquaintance with research atmosphere, he will be vastly rewarded.

The author has evidently read very widely, at least in the older literature, for more than 1100 numbered footnote references are included. There must be as many chapter-end references, which often are not arranged either chronologically or alphabetically.

To the prospective reader, then, I would recommend Taylor's book as a compendium of interesting incidents and cases but distinctly not as an introduction to contemporary dynamic or abnormal psychology.

JOHN F. DASHIELL

Department of Psychology, University of North Carolina

An American in Europe: The Life of Benjamin Thompson, Count Rumford. Egon Larsen. Philosophical Library, New York, 1953. 224 pp. $4.75.

BENJAMIN THOMPSON, Count Rumford, is known as the scientist whose work on heat was revolutionary, and who first demonstrated the convertibility of energy. This book gives a picture of the man, in many ways personally obnoxious, who was none the less a genius, and who served the public welfare in eighteenth century England and Bavaria with lasting distinction. The book is simple, brief, and extremely interesting. It gives the facts directly, and describes Rumford against the changing backdrop of the American Revolution, the Napoleonic Wars, and the French Revolution.

Born near Boston two hundred years ago, Thompson early developed habits of self-discipline that played their role in his emergence as scientist, inventor, and administrator. Handsome, daring, he lived by his early maxim never to allow an opportunity for advancement to escape him. A majority in the Second New Hampshire Regiment was followed by a period of doubtful loyalty to the Revolutionary cause, activity as a secret agent for the Crown, an escape to London, and appointment as "Secretary of the Province of Georgia." Three years after his arrival in London he was elected to the Royal Society.

After being knighted by George III, he went to Bavaria where he became in time Minister of War, Minister of Police, Major General, State Councillor, and Imperial Count. In Munich he established a Workhouse for beggars (there were 2600 in a town of 60,000), a model institution that took beggars off the streets, and provided "warm rooms, a good warm dinner every day, and work for all who were in condition to labour." The feeding of the inmates was based on Thompson's scientific findings. His kitchen range introduced new standards in the efficient use of heat. His pet creation, a potato soup, is still popular in Germany, as Rumford soup, and played its part in popularizing the potato in England. In Munich, too, he built the English Garden, thus converting a wild, neglected marsh along the Isar into a beautiful park.

Returning to England, he was largely responsible for the establishment of the Society for Bettering the Conditions of the Poor, and for the founding of the Royal Institution. This was conceived by Rumford as a center to teach the application of scientific knowledge to the "useful purposes of life;" it became a center associated with some of the most illustrious names in British science.

Thompson died in Auteuil in 1814. He was a great scientist and a great philanthropist. But in his personal life he was arrogant and selfish, and in his philanthropy he was always condescending. A fascinating man.

MINA REES

Dean of Faculty
Hunter College of the City of New York

Curious Creatures. Erna Pinner. Philosophical Library, New York, 1953. 256 pp. Illus. $4.75.

THERE is a whole class of books about natural history produced by what has been referred to as the "Oh My School" of naturalists. Such books have titles like "Marvels of Nature," "Wonders of Animal Life," or "Animal Curiosities." *Curious Creatures* is an

extreme of this type. Even a museum curator's tolerance for emphasis of the more spectacular developments of evolution, which often enough do illuminate some zoological problem or process, is outraged by the abruptness of Miss Pinner's transitions from one marvel to the next. For example: "In contrast to the Water spider, the Small Spotted Dogfish . . . does not build an actual nest" or "It is a far cry from the elegant and attractive Harvest Mouse to the humble Dung Beetle" or "It is a long jump from the African Short-Headed Frog to the South American Water Tortoise." Thus we are made to pass from one animal phylum to another and from one continent to one on the other side of the globe, from one incongruous juxtaposition to the next.

Most of the curiosities of structure or habit set forth and illustrated are reasonably authentic, but precisely in my own field of competence (herpetology) there are curious misinterpretations in both text and figures. The male Darwin's frog is said to have a pair of external vocal sacs, and to take up the eggs into them, as brood pouches. Actually, the vocal sac is single and internal and the tadpoles are taken up into it after they have hatched. The drawing to accompany this phenomenon is evidently imaginary. Equally imaginary is the drawing of a basilisk lizard in its bipedal running position. This lizard assumes a beautiful and natural pose in running on its hind legs, with the long tail extended as a balancer. The source is mentioned for some of the figures drawn directly from famous photographs, but the list should have included Brehm's Tierleben. It must be observed that the copies are much the best of the illustrations. The account of the Carolina box turtle is curiously garbled, and supplied with a figure of a water-tortoise. As for the bat that came to rest on the back of a bull frog, the author appropriately remarks that "Behaviour, like structure, presents many different and speculative features which often appear to the uncomprehending humans nothing more than mysterious phenomena of Nature which remain unanswered riddles exciting our curiousity."

The author and editor were extremely ill-advised in their capitalization of common names of animals, which gives the whole text an extraordinary appearance of being addressed to juvenile readers. In fact, much the best thing to be said for this new compilation is that it is suitable for young children whose parents are not zoologists.

Karl P. Schmidt
Chicago Natural History Museum

Sounds of the American Southwest. Science Series, No. FPX 122. 12-in. double-faced record 33⅓ R.P.M. Folkways Records and Service Corp., New York, 1954. $6.95.

It is most gratifying that Charles M. Bogert followed his ecologist's instincts in making this record instead of confining his interests to the voices of frogs and toads, which belong to his special field of herpetology. He put into his tape recording the noises characteristic of the Chiricahua Mountain region, noises made by mammals, birds, frogs (including toads), rattlesnakes, and beetles, not forgetting to include the whirr of a hummingbird's wings, the crashing roll of thunder in the mountains, and the rush of a flash flood in a mountain canyon.

Moving on to Tucson, he added other animal sounds, and then still more frog voices from California. These turned up what was certainly to be expected—distinctive and constant differences between the voices of forms so closely related as to have been assigned to the same species. Thus the canyon tree frog of Arizona has a voice resembling the trill of the common eastern tree frog; and what was supposed to be the same canyon tree frog in California gives forth a simple succession of quacks.

Bogert has written a carefully scientific commentary to accompany the record, discussing the meaning and function of the animal sounds recorded. It is quite evident that we have entered a new era of study, especially in ornithology and herpetology, in which a tape recorder becomes a part of the equipment of the field naturalist, and records of bird and frog voices must be added to the libraries of those who study these creatures.

Karl P. Schmidt
Chicago Natural History Museum

The Mockingbird Sings. 10-in. double-faced record 78 R.P.M. Cornell Univ. Records, Ithaca, N.Y. $2.50.

It is more than 40 years since I first heard a bird voice recorded for the phonograph, in what probably was one of the earliest attempts at bird-voice recording. It was the song of a caged European nightingale, the bird so famous in the literature for its melodious voice. I was astonished to learn that the thrushes of our Wisconsin forest, with so much less reputation, were so much superior as songsters.

The improvements in sound-recording apparatus that now make possible records of the songs of wild birds have brought about a real enrichment of natural history. Among American songbirds, the first to be singled out for an individual record is appropriately the mockingbird. The mockingbird's vocal performance and capacity for imitating the songs of other birds give it its name, and its literary reputation in America almost corresponds to that of the nightingale in Europe.

To naturalists it is the more appropriate to single out the mockingbird for this distinction because it is so extraordinarily widespread. Thus, the voice of a western subspecies is represented in the new Asch record, *Sounds of the American Southwest,* recorded by my colleague, Charles M. Bogert. The range of the mockingbirds (using the name for a group of forms readily recognized as closely related) extends over two-thirds of the United States, from the Atlantic to the Pacific, and thence southward where various races and closely related species are known through most of Mexico, Central America, and South America. W. H. Hudson writes of hearing it in Patagonia, where the native mockingbird seemed to him to be the premier songster, as ours is to us. Our eastern mockingbird appears to have spread steadily northward since the turn of the century.

Even this record, with its natural song of the mockingbird and 28 imitations of other birds, does not exhaust the range of our songster's capacities, which include medleys of barnyard sounds and even the squeaking wheel of a wheelbarrow.

Karl P. Schmidt

Chicago Natural History Museum

Florida Bird Songs. 10-in. double-faced record, Comstock Publ. Associates, Ithaca, N.Y. $2.50.

Included, with commentaries, are the mockingbird, cardinal, Florida wren, bluejay, boat-tailed grackle, ivory-billed woodpecker, Florida sandhill crane, limpkin, barred owl, and chuck-will's widow.

The Macmillan Wild Flower Book. Clarence J. Hylander. Illus. by Edith Farrington Johnston. Macmillan, New York, 1954. xv + 480 pp. Illus + plates. $15.

MODERN botany began with the herbals, but like the children of immigrant parents, ashamed to be caught speaking the old mother-tongue, the modern botanist—at least when he is teaching his subject to beginners—is inclined to slight mother-tongue. Intent on teaching the newer more fashionable language of genes, mitochondria, and intermediate metabolism, he skims over the material that most people want most to know about plants—what they look like and what they are called. Now there is nothing wrong with the details of biochemistry, genetics, and microscopic anatomy; they are profoundly important matters of knowledge in the world of today. But, except for the specialist, what is taught about them is largely symbolic and often several removes from the phenomena themselves. As a p[illegible]t of departure for the aesthetic appreciation [illegible]e and even the beginning of scientific wisdom, [illegible]hole visible, tangible living plant is better. The instinct to understand first those things we can see, hear, feel, taste, or smell is the stuff science must begin with.

It is comforting therefore to note that the herbal, suitably improved by our modern knowledge and skill minus its original utility as a pharmocopeia, is still with us. In 1953 appeared the *Wildflowers of Western Pennsylvania and the Upper Ohio Valley,* by Jennings and Avinoff, a superb work in the magnificent tradition. Appropriately it was done in Pittsburgh which houses one of the world's great collections of botanical illustration, that of Mrs. Roy A. Hunt. Its cost ($60) however places it out of reach of the usual amateur. Also published in 1953 was the more modest *Wildflowers of America,* by Ricketts, a colored assembly of the more conspicuous and attractive conifer and angiosperm flowers one is likely to meet in various parts of the country. Now comes the *Macmillan Wild Flower Book,* with foreword and brief descriptive material by Clarence J. Hylander and colored illustrations by Edith Farrington Johnston. It is confined to herbs and flowering shrubs, essentially within the range of the latest edition of *Gray's Manual,* whose nomenclature it follows.

The introduction is a brief statement of the more essential information about plant parts and their modifications of the kind that was once widely available in schools using Gray's *How Plants Grow.* The descriptions are of paragraph length, clearly and tersely written, and emphasize the more obvious field characters and give general ranges.

The plates are excellent, lifelike, clearly recognizable, and beautiful from the standpoint of design. The limitations of water-color painting, rather than colored drawings, combined with the somewhat mellow effect of the color process used and the absence of enlarged detail figures may not satisfy the professional taxonomist. They are not intended to. He has facilities of his own. For the purposes of the book, the illustrations seem to me to be ideal. They convey an authentic impression of the plant as a whole, what the average botanist himself sees and uses unless he resorts to hand lens and laboratory. For the traveler, the novice, and even as an *aide-memoire* to the professional, *The Macmillan Wild Flower Book* will have an important place. It furnishes the kind of material with which plant science must start if it is to win friends and neophytes. My impression is that it can use both.

Paul B. Sears

Conservation Program,
Yale University

Die Botanische Buchillustration. Ihre Geschichte und Bibliographie. Claus Nissen. Hiersemann, Stuttgart, 1951. vii + 688 pp. (2 vols. bound together). DM 120.

PUBLICATION of this erudite work, the preparation of which occupied its author for 10 years, is an event of note for botanists who make use of the illustrated literature in their field. For the purposes of his study, Nissen has examined the botanical literature in great detail, including even textbooks and periodicals insofar as their scientific worth or the artistic merit of the illustrator warranted.

The first volume details the history of botanical illustration in chronological sequence from antiquity to the present; the more famous illustrators are discussed at considerable length. For modern times, starting approximately with the beginning of the 18th century, the discussion is arranged by countries. The influence of outstanding botanical artists upon one another, their relationships with the scientists with whom they worked, and the part played by various printers and publishers who produced their work are interestingly detailed.

Volume II, the Bibliography, is much more than a compilation. For the main part, it is an index of works arranged by authors, with details concerning the artists, lithographers, printers, and so forth. Also included are valuable bibliographic references for many of the authors. Following this main index, a similar one pertaining to periodical literature is supplied. The total number of works listed approaches 2400 and would seem to

cover very thoroughly the botanical literature illustrated by means other than photographs, although photographic illustration also is given brief consideration.

The work is thoroughly indexed, according to (i) artists, (ii) plants, (iii) countries, and (iv) authors. The second and third of these indexes, listing the works by plant groups covered and regions treated, are perhaps too general and incomplete to be very helpful, but the index to artists in particular is thorough and extremely useful. The entire undertaking has been carried through with the utmost care and demonstrates painstaking scholarship; the resultant work should be on the shelves of botanical bibliophiles as well as on those of all botanical libraries.

A. C. SMITH

Department of Botany, U.S. National Museum
Washington, D.C.

Galatheas Jordomsejling 1950–1952. A. F. Bruun *et al.*, Eds. J. H. Schultz Forlag, Copenhagen, 1953. 306 pp. 1 map (in Danish).

IN this profusely illustrated book, a popular review is given of the background history, the course, and the direct and potential results of the recent Danish biological deep-sea expedition. Following the tradition of its predecessor, *The Dana Expedition 1928–1930,* the popular report on the Galathea cruise has been prepared jointly by the members of the staff. This gives the nineteen-author text a versatility, enthusiasm, and reliability of detail which would hardly have been possible in a work by one man.

In an introductory chapter R. Spärck gives a review of the 200-year historical tradition behind the expedition and of the Danish pioneer work in marine biology, extending from the famous expedition 1761–1767 on board H.M.S. Grönland up to the present.

The ship's commander, S. Greve, continues with a chapter on the ship and the outlines of the journey. The leader of the expedition, A. Bruun, in one chapter presents the purposes of the expedition, among which may be noted the following: investigation of the benthonic fauna in depths larger than 4000 m; collection of pelagic fish and cephalopodes in all levels below 1000 m; quantitative bottom sampling along sections from the coast out into the deep basins in tropical, subtropical, and temperate areas, outside of coasts of different types, with special regard to comparison between the east and west borders of continents; productivity investigations supplemented with plankton collections; investigations of neritic plankton around isolated islands; magnetic measurements in great depths. In another chapter Dr. Bruun describes the faunas of the deep sea, including much of the important new information which has been gathered by the expedition. Other contributions to the volume deal with the hydrographic results, the giant palms of the Seychelle Islands, sea snakes, trawling technic, the animal life of coastal waters, population density on the ocean floor, deep-sea bacteria, the natural history of Rennell Island, an interesting elevated atoll in the Solomon Islands, the bird life of the oceans, ethnographic and earth magnetic investigations, the sea elephants of Campbell Island, and the public and scientific relations of the expedition. One of the most remarkable contributions is the chapter on the measurement of production of organic matter in the sea, written by the inventor of the most modern method for such purposes, E. Steemann Nielsen. Quite large geographical variations in the productivity were found during the expedition, from 40 to 3800 mg carbon per square meter of the sea surface per day. The lowest value was found in the Sargasso Sea and the highest in the upwelling area at the African West Coast, and in the equatorial upwelling zone which runs across the Pacific. The quantitative importance of productivity in the sea is demonstrated by Steemann Nielsen's estimate of the total amount of organic matter synthesized per year in the ocean, of 40,000 million tons, which is of the same order of magnitude as the estimates for the yearly production on the continents.

Although the great number of illustrations and marginal subtitles help make the book legible for non-Scandinavians, it is hoped that a translation into English will appear, and give this interesting book the circulation it deserves.

GUSTAF ARRHENIUS

Scripps Institution of Oceanography
La Jolla, California

Fundamentals of Reservoir Engineering. John C. Calhoun, Jr. Univ. of Oklahoma Press, Norman, 1953. xvi + 417 pp. Illus. $6.

SINCE petroleum plays such a vital role in American life today, this book dealing with the fundamental factors involved in natural underground petroleum reservoirs and oil fields may be of interest to a broad audience of scientifically or technically trained individuals.

The book deals with (i) the nature of the reservoir fluids—gas, oil, and water; (ii) the nature of the reservoir rocks; (iii) the nature and behavior of rock-fluid system; (iv) reservoir principles involving material balances and rate of flow of gas, oil, and water separately and in the presence of one another; and (v) applications to performance of oil wells.

The author starts each portion with a clear discussion of the underlying physical and chemical principles, expresses them in mathematical form, and presents a simplified physical picture to aid in visualizing the concepts and their applications. The derivations of the equations are carefully outlined in detail to provide a good understanding of the limitations and simplifying assumptions involved. Ideal or extreme conditions are often postulated to illustrate the concepts clearly. Simple problems illustrating the quantitative application of the principles appear frequently and are compared with field or laboratory data on petroleum reservoirs and cores. Calhoun makes clear that these principles may help to visualize some of the very com-

phenomena occurring in oil reservoirs, but that voir engineering is still definitely an art and not a ce. The latter parts of the book deal intensively applications of the fundamental principles and epts in interpreting and analyzing oil-field data. t care is shown in outlining the steps and assump- involved.

he material in this book originally appeared in *The and Gas Journal* as a weekly page entitled "Engi- ing Fundamentals," between 1947 and 1951. The erial was not rewritten but was rearranged for bet- organization. This has resulted in considerable tition in some portions. Too, the use of the original ings in reduced scale makes reading of some of the t legends rather difficult. However, some of the etition makes the material easier to follow. No refer- s later than May 1951 are cited. But, since the phasis in the book is upon the fundamental princi- and concepts, the book provides valuable back- nd for anyone interested in studying the more re- t literature.

undamentals of Reservoir Engineering should pro- e interesting reading to scientists, engineers, geolo- s, students, and anyone interested in the conservation one of our vital material resources. It may also stimu- e ideas for fundamental research to fill in some of e gaps in the field now covered only by empirical cor- ations.

George G. Lamb
emical Engineering Department, Northwestern University

gh Altitude Rocket Research. Homer E. Newell, Jr. Academic Press, New York, 1953. xiv + 298 pp. 93 figs. $7.50.

N this country, comparatively few investigators were engaged in the study of the upper atmosphere ior to 1946. Since that date, when a number of Ger- an V-2 rockets became available for research, the cket warhead was renamed "instrument section" and any laboratories took part in a coordinated program obtain fundamental knowledge of our upper atmo- here.

This book, by Homer E. Newell of the Naval Re- arch Laboratory, describes the progress made under e upper air program. The first third of the book vers the various aspects associated with the use of V-2, Aerobee, and Viking rockets as research vehicles d includes details on rocket performance, instru- entation, and recovery of data, as well as limitations d difficulties. The remainder of the book is devoted o experimental results covering various areas of spe- alization such as ionosphere, solar radiation, and cos- ic rays. For this portion, the author has drawn his aterial almost entirely from the "open literature" pub- shed up to 1953. The author gives reasons for not re- erring to the numerous preliminary reports, not readily ccessible to the reader. Although this ground rule may ot give sufficient credit to some investigators, the reviewer believes that it is a reasonable rule as most of the research work under the upper air program is unclassified.

The book is particularly valuable to the general scientific reader, as he is given a unified presentation of the rocket upper air program and is led directly to specific published data through some background information and detailed descriptions of specific experiments. On the whole, the author has not attempted to combine these results with other observations so that it may be necesary for the reader to refer to other literature in order to obtain a more complete picture of the upper atmosphere. The book ends rather abruptly, with a short chapter on high-altitude photography; a concluding chapter sumarizing the present efforts and some of the unsolved problems would have been desirable.

The publication is a timely one because our earlier enthusiasm of rocket upper air research has declined somewhat, and recently there have been signs of reduction in the upper air program.

Kenichi Watanabe
Air Force Cambridge Research Center
Air Research and Development Command

The Sun. The Solar System, Vol. 1. Gerard P. Kuiper, Ed. Univ. of Chicago Press, Chicago, 1953. 745 pp. Illus. $12.50.

"THIS volume is the first of four intended to give a systematic and comprehensive account of our present knowledge concerning the sun and the other members of the solar system." Thus opens the Preface to one of the most ambitious, scientific, encyclopedic projects recently undertaken. If the caliber of the present volume is indicative of the whole series of four volumes, the scientific fraternity will be highly indebted to Gerard P. Kuiper and his "fifty-six authors in ten countries."

The present volume consists of eight major chapters, each written by one author (or two in the case of Chapter 7) and a composite ninth chapter with brief sections by twelve authors. The introductory chapter by Leo Goldberg is a masterly, semi-historical survey of solar research; it serves in an excellent fashion the dual purpose for which it is written, *viz.* to introduce the physicist, chemist, meteorologist, or scientist in other related fields, to the whole field of modern solar research and to remind the astronomer not especially concerned with solar research of the rapid developments that have taken place in the solar field during the past quarter of a century. This chapter, as all others, has at the end a very useful bibliography.

Chapter 2 by Bengt Strömgren deals with the sun as a star. It reminded me of the 1937 classic by Strömgren in Volume 16 of the *Ergebnisse der Exakten Naturwissenschaften* and it provides a concise and very welcome survey of the problem of energy generation. The lower atmosphere of the sun, the photosphere, is discussed in Chapter 3 by M. Minnaert. This chapter

is written in the clear and illuminating style that astronomers have long appreciated in Minnaert's scientific writings. It is followed by a brief, precise chapter by Charlotte E. Moore, which summarizes our present knowledge of identification of lines in the solar spectrum.

The fifth chapter, by H. C. van de Hulst, deals with two related topics, the chromosphere and the corona. Here is the field of solar research in which the most startling advances have been made in the course of the past twenty years. From the observational side, we have witnessed the development of the coronagraph by the late Bernard Lyot (to whom the 23 authors and the editor deservedly dedicate this first volume), whereas our whole interpretation of coronal phenomena has been affected by Bengt Edlén's discovery of temperatures of the order of one-million degrees in the tenuous outer layers of the sun's atmosphere. Van de Hulst's summary is first-rate and the five sections with his "conclusions" regarding the present status of research will be read with great interest by outsiders as well as by workers in the field.

The chapter on solar activity, by K. O. Kiepenheuer, is the longest in the book and deals expertly with a broad area of solar research, which bears directly on related fields such as geophysics, radio-communication, and the study of the upper atmosphere of the earth. This chapter contains one of the finest collections of photographs relating to solar activity that has ever been assembled.

Chapter 7, by J. L. Pawsey and S. F. Smerd, provides a fine summary of the research done to date in the youngest field of solar studies. The authors point out that, in spite of the fact that "solar radio astronomy is only twelve years old," it has already contributed significantly to the temperature problem of the corona, our knowledge of the (fluctuating or nonexistent!) general solar-magnetic field, electron densities and temperatures in the lower corona and upper chromosphere; the study of the radio-sun has opened up entirely new vistas for research in solar activity. In future solar research, studies by optical and radio methods must go together.

The concluding major chapter, by T. G. Cow[ling] is a brief but much needed introduction to the [f...] of solar electrodynamics. It is becoming abunda[ntly] clear that the basic concepts of magneto-hydro[dy]namics will play increasingly more important role[s in] the interpretation of solar phenomena, but it has b[een] difficult for the uninitiated to follow the developm[ents] in this area of theoretical research. Cowling's cha[pter] shows the way in an unpretentious yet thorough fash[ion].

I cannot shower on Chapter 9 the variety of pr[aise] which seemed justified for Chapters 1 through 8. W[ith] twelve authors and thirteen sections of varying lengt[h it] is concerned with empirical problems and equipm[ent]. The separate sections are written by the best men [in] the field and each is well worth reading, but the t[otal] picture is somewhat scattered and confused. I am s[orry] that Dr. Kuiper did not see fit to ask two or three [of] the present twelve authors to share the responsibi[lity] of writing one or two first-rate chapters on instrumen[ta]tion and observational technics relating to solar [re]search. I have no faults to find with the contributi[ons] by the separate authors, which are almost without [ex]ception first-rate, but I wish that the thirteen secti[ons] in manuscript form might have been the notes fr[om] which one or two really comprehensive chapters wo[uld] have been written.

In concluding this review, I can state without reser[va]tion that the present volume deserves a very wide d[is]tribution. No astronomer, solar physicist or otherw[ise] can afford to be without this volume and the physic[ists,] chemists, geophysicists, and geologists cannot ask [for] a more readable and comprehensive volume. Gradu[ate] students with a minimum of background in phys[ics] and mathematics should not find it difficult to stu[dy] any one of the chapters and the use of the volume [as] a reference book (or textbook) in advanced courses [is] to be recommended. In other words, *The Sun* deser[ves] a place in most scientific libraries, personal and [in]stitutional.

BART J. B[OK]

Harvard College Observatory
Harvard University

The Republic *Does* Have Need of Savants!

The responsibilty for forming ideals and fixing standards does not belong to statesmen alone. It belongs, and now perhaps more largely than ever before, to the intellectual leaders of the nation, and especially to those who address the people in the universities and through the press. Teachers, writers, journalists are forming the mind of modern nations to an extent previously unknown. Here they have opportunities such as have existed never before, nor in any other country, for trying to inspire the nation with a love of truth and honor, with a sense of the high obligations of citizenship, and especially of those who hold public office.—JAMES BRYCE.

Meetings

ber

International Cong. of Chronometry, Paris. (R. Baillaud, Observatoire National, Besançon, France.)

International Cong. on Diseases of the Chest, 3rd, arcelona. (American College of Chest Physicians, 112 Chestnut St., Chicago 2.)

Southern Psychiatric Assoc., Louisville, Ky. (G. utherland, 221 N. Charles St., Baltimore.)

World Medical Cong. of Homeopathy, 1st, Rio de aneiro, Brazil. (R. Seidel, 1801 Porter St., Philaelphia.)

Electrochemical Soc., autumn, Boston. (H. B. Linrd, Columbia University, New York 27.)

National Electronic Cong., 10th annual, Chicago. K. Kramer, 852 E. 83 St., Chicago 19.)

International Plastics Cong., 6th, Turin, Italy. (M. uzzoli, Salone Internazionale della Tecnica, 20 Via assena, Turin.)

3. International Telecommunication Union, 17th, eneva, Switzerland. (Sec., ITU, Palais Wilson, eneva.)

American Inst. of Electrical Engineers, Middle astern District, Reading, Pa. (H. H. Henline, 33 W. 9 St., New York 18.)

International Cong. of Hydatid Diseases, 5th, Marid. (J. C. Melendro, Hospital Provincial, Sorea, pain.)

4. Conf. on Weights and Measures, 10th, Sèvres, rance. (Sec., Bureau International des Poids & Meures, Pavillon de Breteuil, Sèvres.)

Inst. of Radio Engineers, Professional Group on uclear Science, 1st annual, Chicago. (T. Fields, V.A. ospital, Hines. Ill.)

Commercial Chemical Development Assoc., annual all, Bedford, Pa. (W. T. Hack, Ethyl Corp., 100 Park Ave., New York 17.)

Pan American Homeopathic Medical Cong., 25th, io de Janeiro, Brazil. (H. O. Skinner, 2134 Grand Ave., St. Paul 5, Minn.)

2. Pan American Sanitary Cong., Santiago, Chile. M. E. Bustamante, Pan American Sanitary Bureau, ashington 6, D. C.)

Acad. of Psychosomatic Medicine, 1st annual, New ork City. (E. A. Brown, 75 Bay State Rd., Boston 15.)

12. Ferrimagnetism Conf., Silver Spring, Md. (L. R. axwell, U. S. Naval Ordnance Laboratory, Silver pring.)

13. American Oil Chemists' Soc., 28th annual fall, inneapolis, Minn. (L. R. Hawkins, AOCS, 35 E. acker Dr., Chicago 1.)

14. Cong. of International Anesthesia Research Soc., os Angeles. (T. H. Sheldon, 102–110 Second Ave., ochester, Minn.)

15. American Public Health Assoc., Buffalo, N. Y. R. M. Atwater, 1790 Broadway, New York 19.)

17. Audio Engineering Soc., annual, New York City. (C. J. LeBel, AES, P.O. Box 12, Old Chelsea Station, New York 11.)

15. Bicentennial Conf. of the Columbia University ollege of Pharmacy, New York City. (G. W. Merck, Merck & Co., Rahway, N. J.)

16. Electron Microscope Soc. of America, Highland Park, Ill. (J. R. Cooper, EMSA, General Electric Co., Cleveland 12, Ohio.)

14–16. Indiana Acad. of Science, Lafayette, Ind. (W. A. Daily, Eli Lilly & Co., 740 S. Alabama St., Indianapolis 6.)

14–16. Optical Soc. of America, annual, Los Angeles. (A. C. Hardy, Massachusetts Institute of Technology, Cambridge 39, Mass.)

15–17. International Colloquium on Dermatology and Syphilography, 5th, Marseilles, France. (J. Bonnet, Hotel-Dieu, Place Daviel, Marseilles.)

16–25. International Cong. of Homeopathy, 18th, Rio de Janeiro, Brazil. (A. Benjamin, 46 Bickenhamm Mansions, Gloucester Pl., London, W.1.)

18–20. Conf. on Electrical Insulation, Pocono Manor, Pa. (A. McLean, 2101 Constitution Ave., Washington 25, D. C.)

18–20. IRE-RETMA, fall, Syracuse, N. Y. (E. K. Gannett, 1 E. 79 St., New York 21.)

18–21. Mental Hospital Inst., 6th, Minneapolis, Minn. (W. Malamud, 80 E. Concord St., Boston 18, Mass.)

18–22. American Soc. of Civil Engineers, New York City. (W. M. Carey, 33 W. 39 St., New York 18.)

18–22. Soc. of Motion Picture and Television Engineers, semi-annual, Los Angeles, Calif. (SMPTE, 342 Madison Ave., New York 16.)

19. American Soc. of Safety Engineers, Chicago. (J. B. Johnson, 425 N. Michigan Ave., Chicago 11.)

21–22. National Noise Abatement Symposium, 5th annual, Chicago, Ill. (S. M. Potter, Illinois Inst. of Technology, Chicago 16.)

21–23. International Assoc. of Milk and Food Sanitarians, Atlantic City, N. J. (H. L. Thommasson, IAMFS, Box 437, Shelbyville, Ind.)

21–23. International Symposium on the Dynamics of Virus Infections, Detroit, Mich. (Henry Ford Hospital, 2799 W. Grand Blvd., Detroit 2.)

21–24. American Dietetic Assoc., annual, Philadelphia. (E. A. Atkinson, 620 N. Michigan Ave., Chicago 11.)

22–26. Symposium on Wind and Solar Energy, New Delhi, India. (UNESCO, 19 Ave. Kléber, Paris 16.)

24–27. Soc. of American Foresters, Milwaukee. (H. Clepper, 425 Mills Bldg., Washington 6, D. C.)

25–27. Assoc. of American Medical Colleges, Bedford Springs, Pa. (AAMC, 5 S. Wabash Ave., Chicago 3.)

25–30. International Cong. of Odontology, 1st, São Paulo, Brazil. (F. Degni, Rua Marconi 131, São Paulo.)

26. Assoc. of Consulting Chemists and Chemical Engineers, annual, New York City. (A. B. Bowers, 50 E. 41 St., New York 17.)

27–29. International Symposium on the Hypophyseal Growth Hormone. Its Nature and Actions, Detroit, Mich. (R. W. Smith, Henry Ford Hospital, Detroit 2.)

28–30. American Soc. for Aesthetics, Bloomington, Ind. (R. R. Patrick, Cleveland Museum of Art, Cleveland 6, Ohio.)

28–30. International Symposium on Temperature, Washington, D. C. (W. Waterfall, 57 E. 55 St., New York 22.)

31–1. American Soc. for the Study of Arteriosclerosis, annual, Chicago. (A. C. Corcoran, Cleveland Clinic, Cleveland 6, Ohio.)

New Books Received

Scientific and Technical Papers. Seinen Yokota. Comp. and pub. by the Yokota Memorial Com. Univ. of Tokyo, 1954. xx + 398 pp. + 137 tables. Illus.

Chemistry of the Defect Solid State. A. L. G. Rees. Methuen, London; Wiley, New York, 1954. viii + 136 pp. Illus. $2.

Advances in Cancer Research. vol. II. Jesse P. Greenstein and Alexander Haddow. Eds. Academic Press, New York, 1954. xi + 530 pp. Illus. $11.

Cancer of the Lung: A Symposium. Johs. Clemmesen, Ed. Council for International Organisations of Medical Sciences, Paris, 1953 (Reprinted from ACTA). 210 pp. Illus. $6.

Geometrical Mechanics and De Broglie Waves. J. L. Synge. Part of Cambridge Monographs on Mechanics and Applied Mathematics, G. K. Batchelor and H. Bondi, Eds. Cambridge Univ. Press, New York, 1954. vi + 167 pp. Illus. $4.75.

The Nature of Light and Colour in the Open Air. Rev. ed. M. Minnaert. Trans. by H. M. Kremer-Priest; rev. by K. E. Brian Jay. Dover, New York, 1954 (Reprint of earlier ed.). xi + 362 pp. Illus. + plates. Cloth, $3.95; paper, $1.95.

The Properties of Glass. ed. 2. George W. Morey. ACS Monogr. Ser. No. 124, William A. Hamor, Ed. Reinhold, New York, 1954. 591 pp. Illus. $16.50.

Home Repairs and Improvements Emanuele Stieri. Barnes & Noble, New York, 1954 (Reprint of 1950 ed.). vii + 375 pp. Illus. Paper, $1.50.

Physics Principles. Stanley S. Ballard, Edgar P. Slack, and Erich Hausmann. Van Nostrand, New York–London, 1954. vi + 743 pp. Illus. $7.50.

Monomolecular Layers. A symposium presented at the Philadelphia meeting of the AAAS. Harry Sobotka, Ed. AAAS, Washington, D. C., 1954. vii + 207 pp. Illus. $4.25 (Members, $3.75).

The Principles of Physical Optics. An historical and philosophical treatment. Ernst Mach. Trans. by John S. Anderson and A. F. A. Young. Dover, New York, 1953 (Reprint of 1926 ed.). x + 324 pp. Illus. + plates. Cloth, $3.50; paper, $1.75.

Physical Meteorology. John C. Johnson. Technology Press, M. I. T., Cambridge; Wiley, New York; Chapman & Hall, London, 1954. xii + 393 pp. Illus. $7.50.

The Neolithic Cultures of the British Isles. Stuart Piggott. Cambridge Univ. Press, New York, 1954. xix + 420 pp. Illus. + plates. $13.50.

The Present State of Physics. A symposium presented on Dec. 30, 1949 at the New York meeting of the AAAS. Arranged by Frederick S. Brackett. AAAS, Washington, D. C., 1954. vi + 265 pp. Illus. $6.75 (Members, $5.75).

V-2. Walter Dornberger. Trans. by James Cleugh and Geoffrey Halliday. Viking Press, New York, 1954. xviii + 281 pp. + plates. $5.

Minnesota's Rocks and Waters. A geological story. George M. Schwartz and George A. Thiel. Univ. of Minnesota Press, Minneapolis, 1954. xviii + 366 pp. Illus. $4.

Essays on the Social History of Science. S. Lilley, Ed. Munksgaard, Copenhagen, 1953. 182 pp. Paper, 30 kr.

Time Counts. The story of the calendar. Harold Watkins. Philosophical Library, New York, 1954. vi + 274 pp. Illus. + plates. $3.75.

Methods of Research. Educational, psychological ciological. Carter V. Good and Douglas E. S Appleton-Century-Crofts, New York, 1954. xx pp. $6.

Advances in Enzymology and Related Subjects Biochemistry. vol. XV. F. F. Nord, Ed. Intersc New York–London, 1954. x + 547 pp. Illus. $11.

The Sophists. Mario Untersteiner. Trans. by Kat Freeman. Philosophical Library, New York, 1954. 368 pp. $6.

Progress in Biophysics and Biophysical Chemi vol. 4. J. A. V. Butler and J. T. Randall, Eds. demic Press, New York; Pergamon Press, Lon 1954. viii + 339 pp. Illus. + plates. $9.50.

Induction and Dielectric Heating. J. Wesley C Reinhold, New York, 1954. vii + 576 pp. Illus. $1

The Microtomist's Formulary and Guide. Peter G Blakiston, New York, 1954. xiii + 794 pp. Illus. $1

Applied Atomic Energy. K. Fearnside, E. W. J and E. N. Shaw. Philosophical Library, New Y 1954. viii + 156 pp. Illus. + plates. $4.75.

Heat Conduction. With engineering, geological, other applications. Leonard R. Ingersoll, Otto J. Z and Alfred C. Ingersoll. Univ. of Wisconsin P Madison, rev. ed., 1954. xiii + 325 pp. Illus. $5.

Method and Perspective in Anthropology. Paper honor of Wilson D. Wallis. Robert F. Spencer. Univ. of Minnesota Press, Minneapolis, 1954. 323 pp. $4.50.

Plant Life in Malaya. R. E. Holttum. Longmans, Gr London–New York, 1954. viii + 254 pp. Illus. $

Die Entwicklung und Morphologie des Chondrok iums von Myotis Kaup. Hans Frick. Georg Thie Stuttgart; Intercontinental Medical Books, New Y 1954. 102 pp. Illus. Paper, $3.45.

An Introduction to Bacterial Physiology. Evelyn Oginsky and Wayne W. Umbreit. Freeman, San Fr cisco, 1954. xi + 404 pp. Illus. Text, $6; trade, $7

Human Development. John P. Zubek and P. A. Solb McGraw-Hill, New York–London, 1954. vii + 476 Illus. $6.

The Kachina and the White Man. A study of the fluences of white culture on the Hopi Kachina c Frederick J. Dockstader. Cranbrook Institute of S ence, Bloomfield Hills, Mich., 1954. xiv + 185 pp. Il + plates. $5.

Basic Botany. An introduction to the science of bota Fred W. Emerson. Blakiston, New York, ed. 2, 19 xiii + 425 pp. Illus. $5.

The Development of Medical Bibliography. Est Brodman. Medical Library Assoc., Baltimore, 1 (Order from Archives Curator, Medical & Chirurg Faculty of State of Maryland, Baltimore 1). ix + pp. Illus. + plates. $5.

Nature and Nurture: A Modern Synthesis. John Fuller. Doubleday, Garden City, N. Y., 1954. vi + pp. Paper, $0.85.

The Alkaloids: Chemistry and Physiology. vol. I R. H. F. Manske and H. L. Holmes, Eds. Acader Press, New York, 1954. x + 357 pp. $8.50.

Contributions to American Anthropology and Histo vol. XI, No. 52–56. Carnegie Institution, Washingto 1952. 236 pp. Illus. + plates. Cloth, $7.50; Pap $6.75.

The Structures and Reactions of the Aromatic Compounds. G. M. Badger. Cambridge Univ. Press, New York, 1954. xii + 456 pp. Illus. $11.50.

The First Australians. Ronald M. Berndt and Catherine H. Berndt. Philosophical Library, New York, 1954. 144 pp. Illus. + plates. $4.75.

Introduction to Mathematical Statistics. Paul G. Hoel. Wiley, New York and Chapman & Hall, London, ed. 2, 1954. xi + 331 pp. Illus. $5.

Abnormal and Pathological Plant Growth. Report of Symposium held 3–5 Aug. 1953. Brookhaven National Laboratory, Upton, N.Y., 1954 (Order from Office of Technical Services, U.S. Dept. of Commerce, Washington 25). vii + 303 pp. Illus. Paper, $2.10.

Encyclopédie Entomologique. Catalogue Illustré des Lucanides du Globe. R. Didier and E. Séguy. Atlas with text (1952). 112 plates. Paper, 8000 fr. Text with atlas (1953). 223 pp. Illus. Paper, 8000 fr. Lechevalier, Paris.

Précis de Minéralogie. P. Lapadu-Hargues. Masson, Paris, 1954. 311 pp. Illus. + plates. Cloth, 2200 fr.; paper, 1700 fr.

The Meaning of Social Medicine. Iago Galdston. Harvard Univ. Press, Cambridge, 1954 (for the Commonwealth Fund). viii + 137 pp. $2.75.

Fishes of the Marshall and Marianas Islands. vol. 1. Families from Asymmetrontidae through Siganidae. Leonard P. Schultz *et al.* Smithsonian Institution, Washington, 1953 (Order from Supt. of Documents, GPO, Washington 25). 685 pp. Illus. + plates. Paper $2.75.

Psychological Testing. Anne Anastasi. Macmillan, New York, 1954. xiii + 682 pp. Illus. $6.75.

Nuclear Theory. Robert G. Sachs. Addison-Wesley, Cambridge, Mass., 1953. xi + 383 pp. Illus. $7.50.

Principles of Biochemistry. A biological approach. M. V. Tracey. Pitman, New York–London, 1954. ix + 194 pp. Illus. $4.

Newer Concepts of the Causes and Treatment of Diabetes Mellitus. Proc. of the symposium on diabetes held 8 Oct. 1953. National Vitamin Foundation, New York, 1954. iv + 181 pp. Illus. Paper, $2.50.

The Causes and Treatment of Backwardness. Cyril Burt. Philosophical Library, New York, 1953. 128 pp. $3.75.

Léonard de Vinci et l'Expérience Scientifique au XVI^e Siècle. Centre National de la Recherche Scientifique. Presses Universitaires, Paris, 1953. viii + 273 pp. Illus.

Autotrophic Micro-Organisms. Fourth symposium of the Society for General Microbiology held in April 1954. B. A. Fry and J. L. Peel, Eds. Cambridge Univ. Press, New York, 1954. ix + 305 pp. Illus. $5.

Histology. Roy O. Greep, Ed. Blakiston, New York, 1954. xi + 953 pp. Illus. + color plates. $15.

Henri Poincaré: Critic of Crisis. Reflections on his *Universe of Discourse.* Tobias Dantzig. Scribner's, New York–London, 1954. xi + 149 pp. $3.

A Curriculum for Schools of Medical Technology. Israel Davidsohn and Kurt Stern, Eds. Registry of Medical Technologists, American Society of Clinical Pathologists, Muncie, Ind., ed. 3, 1953. 122 pp. Illus. Paper, $3.

L'Analyse Spectrale Quantitative par la Flamme. pt. I, *Propriétés de la flamme, réalisation et utilisation;* pt. II, *Analyse des émissions dans la flamme.* R. Mavrodineanu and H. Boiteux. Masson, Paris, 1954. 247 pp. Illus. Cloth 4300 fr.; paper, 3800 fr.